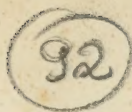


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THE



DUBLIN JOURNAL

OF

MEDICAL AND CHEMICAL SCIENCE;

EXHIBITING

A COMPREHENSIVE VIEW

OF THE

LATEST DISCOVERIES

IN

MEDICINE, SURGERY, CHEMISTRY, AND THE COLLATERAL
SCIENCES.

VOL. III.

DUBLIN:

PUBLISHED BY HODGES AND SMITH,

21, COLLEGE-GREEN,

LONGMAN, REES, AND CO., AND SIMPKIN AND MARSHALL, LONDON;
MACLACHLAN AND STEWART, EDINBURGH; AND
SMITH AND SON, GLASGOW.

1833.

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The papers of Drs. Colvan, Haughton, and Patterson, shall appear in the next Number.

The papers of Mr. Grey and Dr. M'Keever are under consideration.

ERRATA.—No. V.

Page 203, line 11, for reaching *read* retching.

— 205, — 24, for presents *read* presented on Thursday the 14th inst.

— 206, — 1, for Mark *read* Master.

— 210, — 2, for "association." Generally, these experiments, *read* "association, generally. These experiments.

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NOTICE TO CORRESPONDENTS.

We beg to acknowledge the receipt of Dr. Crawford's letter, and are glad to find that the coincidence between some of his remarks on the prognosis in Convulsions, as given in his excellent paper on that subject in the *London Cyclopædia of Practical Medicine*, with those of Jolly in the *Dictionnaire de Médecine et de Chirurgie*, was purely accidental, (see *Dublin Medical Journal*, Vol. II, No. 5.) At the period when Dr. C. wrote his article, he was not even aware of the existence of the essay of Jolly.

THE
DUBLIN JOURNAL
OF
MEDICAL AND CHEMICAL SCIENCE,
1 MARCH, 1833.

PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*Notes on Malignant Cholera, as it appeared in Dublin.* By SIMON M'COY, late one of the Resident Medical Officers of the Grange Gorman-lane Cholera Hospital.

(Continued from Vol. II. page 375.)

I HAVE endeavoured to observe something like arrangement in transcribing my notes for the press, but being taken originally without any, and want of sufficient time at present, frustrates my intention; I may therefore be excused throwing in one or two observations, somewhat out of place, before I come to the treatment of cholera.

The endemic accession of the disease, and its return after an almost total subsidence, have been alike sudden; one or two straggling cases are first observed, like the advanced guard of an army before the main body appears. In its commencement, wherever I witnessed it, the first cases that occurred were of the

worst description, but as it gained ground its symptoms were milder, until at length it became what are popularly called 'simple bowel complaints,' which would have received little attention from the patients themselves at any other period, but some of which I have known assume the malignant form, from, I believe, the treatment they received. I saw this verified twice in Dublin, where, after it had almost totally disappeared, it returned in the same way as in the first instance, but with a still heavier visitation. I witnessed a similar method in its approach and departure likewise in the interior, but even to the last a desperate case will now and then occur. I have lately been for some weeks in the North of Ireland by desire of the Central Board of Health, in charge of two districts affected with cholera, and have had an opportunity of seeing it under somewhat new circumstances. I found a population existing in a state of poverty hardly to be credited by those who have not witnessed it. Most of the peasantry living in huts, the thatched roofs of many of which give as free entrance to the frequent showers, as their unglazed windows admitted the cold air of an inclement December; their food but a few potatoes and some salt, for a scanty morning and evening meal. They were very much alarmed at the presence of the disease among them: this fear occasioned me to be sent for in many trifling stomach and bowel complaints, and an opinion may be formed of their wretchedness, from the cause assigned for most of those occurring on the 25th of December, and two or three following days, namely, the eating of a little flesh meat on Christmas day by those who had not tasted animal food for twelve months before! unwholesome straw, damp earthen floors, steaming mud walls, stinted fuel, and uncleanness, appeared to invite disease, while numerous facilities of obtaining ardent spirits, where other comforts were unattainable, would seem to provoke it—yet these accredited causes of cholera were rather weak here; the cases that occurred were not more intractable than those among the working classes in the metropolis; this was the more remark-

able from the fact, that when these peasantry were attacked by other diseases, the same remedial vigor could not be employed for their relief; they could not bear the same medicines nor the same doses as are daily exhibited to the better clad, better housed and fed artizans in the city. I did not remark any great peculiarity in the symptoms, or the effect of treatment of cholera among them.

I have not seen any *critical* symptoms occur in the recovery from cholera; I have read of the breaking out of warm sweats accompanying the first appearance of re-action, and the return of the secretion of urine has been noted as a crisis; the first evacuations of urine do not often exceed half an ounce at each time, and the increase to the natural quantity is generally very gradual; but all the other symptoms of recovery may precede this evacuation, by hours or even days. I met two cases of cholera, one in private, and the other in the hospital, where the patients had been for a long time afflicted with hemiplegia; both suffered much from cramps, but in both, these affected the paralytic limbs only.

I have stated that very few of the servants of the hospital were attacked with cholera; I sent a good many of my nurses to different parts of the country at the request of gentlemen connected with the local Boards; of those, some who had been for two or three months in the Grange Gorman Hospital in good health, took the disease on their arrival at the country hospital—two of them I heard died in Drogheda alone. The nurses in town had seldom even the ordinary bowel affections, which from the season of the year, and other causes, might have been expected; some of them had simple fever, which was removed in from four to seven days, by shaving the head, leeches to the temples, laxative medicines, whey diet, and rest. In the middle of July several of the younger ward-maids were attacked suddenly with a kind of hysterical affection, as it was called; the symptoms were flushed countenance, abdomen prodigiously distended and tympanitic, hurried and oppressed respiration, rest-

lessness, and sometimes jactitation of the body or limbs, hurried pulse, and great anxiety. The introduction of an elastic tube into the rectum usually gave exit to a large quantity of flatus, and removed much of the uneasiness; a foetid enema and a warm cathartic, with sometimes a few ounces of blood drawn from the arm, set all things to rights in a few hours. These symptoms sometimes, but not always, appeared connected with deranged menstruation. Sickness of some kind might have been anticipated in those females from their confinement, and the great labor, exertion, and want of rest they occasionally endured; when I look back on all I have known them to go through, I am surprised so few of them became ill. I have met two cases of abscess in the sole of the foot, which required to be opened, and then gave no further trouble, and two cases of mortification of the toes, but whether they were connected with cholera or not, I do not know: a medical friend mentioned to me a case where the nose was threatened with mortification in or after cholera.

Every one is familiar with the phrase ‘the Consecutive Fever of the malignant cholera.’ In books and in conversation I have found a remarkable unanimity of opinion to this effect, that without the greatest care and watching, and even sometimes in despite of the medical attendant’s best exertions, a derangement of the system of a pyrexial character will supervene on reaction, and which is little less fatal than cholera itself, and that this is often attended with local inflammation in some of the great cavities. The respect I entertain for the professional acquirements of some who I know entertain this opinion, and the scientific character of some of the works where it is likewise to be found, would make me hesitate in the assertion of a contrary belief, did the question rest on other than plain matter of fact, (for into this every thing I state must resolve itself), but on this account, as well as from its abstract importance, I feel I am not guilty of unseemly presumption when I say, that the supervention of fever, with or without inflammatory engagement of the cranial, thora-

cic, abdominal, or pelvic viscera on the setting in of reaction, is by no means a legitimate consequence of cholera, but I believe the line of practice not unfrequently pursued in its treatment is the chief, if not the only cause of the occurrence of those symptoms which compose the consecutive fever. Should the treatment by stimulants be adopted, great care and watching will indeed be necessary. The instant at which reaction commences must, in a vast majority of cases, pass unobserved, if in fact it be at all appreciable, and pushing stimulants beyond that instant will certainly cause a febrile disturbance. I have known this frequently occur, but I never saw the consecutive fever of cholera as a new and peculiar species, or independantly of the causes assigned ; it has always, as far as I have seen, exhibited only the characters which follow the abuse of opium, brandy, strong diuretics, &c. The brain or its envelopes, the stomach, bowels, kidneys, or bladder, will exhibit signs of inflammation ; the same causes would produce the same effects at any time, and the only peculiarity I have noticed regarding those *inflammations* after cholera is, that they yield I think much more readily to the ordinary antiphlogistic treatment than such cases usually do in every-day practice.

The treatment pursued or recommended for malignant cholera may be divided into three classes, viz., that founded on preconceived opinions, as of the causes and nature of the disease, &c. ; that founded on the known effects of medicines in certain derangements of the animal economy, as exhibited by well defined symptoms appearing to bear an obvious analogy with those of cholera ; that of those who, in despair, try any thing in hopes that something may hit. The revival of making the chemical analysis of the animal fluids as a basis for medical practice, is one of the many remarkable occurrences occasioned by the appearance of cholera among us. The blood of persons labouring under this disease has been analyzed with the most severe scrutiny, and some slight acerbity has been manifested between different experimenters, as to priority of claims to discoveries, and on the difference of results from the same analytic proceeding

in different hands. Some found more carbon in cholera blood than there ought to be, and of course the most obvious mode of extracting it from the living man was by the inhalation of oxygen. I was not present at any trial of this practice, but oxygen and protoxide of azote have, I understood, been administered in Dublin, and the effect did not warrant their repetition. Other experimenters announced their discovery of a *lentor* in cholera blood, or a great diminution of its water and soluble salts; this led to the idea of injecting into the veins warm water, holding in solution certain quantities and proportions of muriate and carbonate of soda and chloride of potash; I could not bring myself (perhaps from old prejudices) to inject a quantity of this mixture nearly equal to the volume of all the blood in the body, as I read has been done, but I witnessed the operation on the two occasions it was performed in the Grange Gorman-lane hospital; four or five pints of water at the temperature of about 112° of Farht., having the usual salts dissolved in it, were slowly thrown into a vein in the bend of the arm; after a few strokes of the piston, by which I should suppose ten or twelve ounces had been injected, the pulse at the wrist of the other arm became a little fuller and accelerated at the rate of about thirteen beats in a minute; until towards the close, the patient did not feel any thing particular from the process, but then he began to complain of a tightness about his chest; he expired half an hour after his arm was tied up, as I learned. In the other case, by some mismanagement, the cellular substance of her arm was extensively injected, but most of the fluid was thrown into the vein; in an hour and a half after I went into the ward where she was, and found her complaining very much of her arm, but of nothing else that could be put to the account of the injection; she died within two hours from the commencement of the trial. I did not by any means consider either of these cases hopeless previously to the injection; in both the pulse was palpable at the wrist; in neither (particularly the female) was the heat of surface greatly reduced; one of them had

a good voice, and the other vomited freely: the conviction that if the operation they underwent had not caused, it certainly much accelerated their dissolution, confirmed my resolution never to try this remedy. Another method advised for supplying the deficiency in the saline ingredients of the blood was through the absorbents of the stomach and intestines; this is called the treatment by the 'non-purgative salts.' Not having very much faith in this proposal I did not deem it prudent to lose time in their trial, but I watched their exhibition in a number of cases under a judicious and careful practitioner in the hospital, and as far as the medicine itself be considered, I do not think any thing more innocent could be given; in fact neither the prescriber, the patients, nor myself, could perceive any effect whatever from their use. Another practitioner of much experience in cholera, who had given the non-purgative salts a fair trial in the hospital, informed me that he had found them equally unsatisfactory. I had a notion that taking sparks with an electric machine from the surface of the body might possibly be of some service, and whether it was my appearing anxious to have this tried or not, I cannot say, but a patient was shortly after treated with electricity; but instead of merely stimulating the surface in the way I imagined, the contents of a charged jar were repeatedly sent through the thorax; from the description of its effects I should not wish to witness a similar experiment; the case was a very bad collapse, no advantage was gained. Pressure upon the surface by means of flannel rollers was tried in one case I witnessed; they were applied with great care by a young gentleman, but failed to do any service. Some have conceived, either from examination after death, or from observations on the state of the circulatory organs in the living, that the proximate cause of cholera is in the heart and blood-vessels; that these parts have become unable to perform their functions from the blood being thrown in unusual quantities on the large trunks, or that they have by some means become inadequate to the propulsion of the usual quantity, and suppose that there are analo-

gous cases were detraction of blood raises the pulse when it has been extremely depressed. That there are cases where bleeding raises the pulse of course is true, but that these cases are like the collapsed stage of cholera is perhaps very doubtful; however blood-letting has been advised, with some difference of opinion as to the precise time it ought to be done; some say in the very commencement, when the patient is falling into collapse; others while in this latter state, and others confine the practice to the purposes of controlling reaction within proper limits, or moderating some of its urgent symptoms. I saw it practiced on a ward-maid half an hour after she exhibited the first symptoms of cholera, but it did not succeed in keeping up the pulse, which fell rapidly as the blood flowed. It so seldom happened to me to see cases before an interval of some hours had elapsed, that few opportunities presented themselves of practicing phlebotomy at this early stage of the disorder, and of those few I did meet, there seemed so little general indication for the use of the lancet, that I never made the *experiment* myself, (for such I would call it,) and the instance I witnessed did not serve to encourage me in its adoption as a practical benefit. A patient in one of my wards complained of great squeezing, as he said, about his heart, and urged me repeatedly to bleed him, as a relative of his who had had cholera recovered after being bled; he had been eleven hours ill, had a small, weak, but very perceptible pulse at the wrist, beating 120 strokes in a minute; he pressed his desire so strongly upon me that I requested the superintendent physician to see him, and advise as to the propriety of acceding to his wishes, which was decided in the affirmative; a vein was accordingly opened, and before three ounces of blood were abstracted I felt the pulse going; the arm was in consequence tied up, but it continued rapidly to decline, and never again rallied. A man was sent into me on the 20th May nearly moribund, without pulse at the wrist, that of the carotid beating 130, great dyspnoea, and cold skin and tongue; he laid his hand over his heart and exclaimed, "Oh, its all here."

I knew he had no chance from ordinary measures, so determined to try if bleeding would give him even temporary relief ; I opened successively a vein in each arm and a temporal artery, but no blood flowed, nor could I by any means induce more to come than a few drops, which of course did neither good nor harm. A medical gentleman of this city took the trouble to explain to me a new method he conceived for the treatment of cholera, and had the kindness to take me to see the apparatus he contrived to effect his purposes. He was of opinion that the languid state of the circulation in the disease might be remedied by the removal, as he expressed himself, of a "few tons" of atmospheric pressure from the surface of the patient's body ; to do this the sick person was to be enclosed in a sheet-iron envelope, having an aperture for his face ; the whole was made air-tight, an exhausting pump or syringe was connected to one side, and a barometrical tube made to communicate with the interior of the case by one end, while the other, immersed in a vessel of mercury, marked the degree of exhaustion. While I staid, a piece of wetted bladder was put over the opening for the face, and the exhausting syringe worked, but by the time the man who worked at it was quite exhausted with the labour, the mercury had not risen one inch in the gage ; of course the exhausting engine could be constructed so as to raise the mercury to any height within the range, that the iron case would be strong enough to bear ; but suppose three tons of atmospheric weight taken off a patient's body (perhaps more than one-fifth of the entire pressure he usually supported,) it is obvious that weight must be thrown on the surface of the air-cells of the lungs ; could they bear it without injury ? or could the mechanical part of respiration be at all carried on under such circumstances ? I do not know, for I have not seen it tried on a living person, but supposing it could be borne, would this not increase one of the worst and most striking features of the disease ? would it diminish the turgescence of the cutaneous blood-vessels ? I must not occupy my space with the conjectures that

crowd on me. A medical friend told me he saw this contrivance tried on a cholera patient, and that it did no good, but the mercury in the gage had not risen half an inch.

When a medical man sees his patient cold, pulseless, powerless, with difficulty of breathing, a stagnant circulation, with purging and vomiting, and cramped limbs, the curative indication may appear very obvious to him—that artificial heat must be applied, cordials and other stimulants administered, bleeding or antispasmodics employed, opiates and astringents had recourse to, or the worst consequences may be expected—and this, with of course considerable variety in the materials, forms nineteen-twentieths of the recorded practice in cholera. It is a melancholy retrospect, that of the results of medical efforts for the amelioration of the afflicted in this dreadful disease; it has confounded experience; it has reversed judgment, and set theory and speculation at utter defiance. Before cholera came to Ireland I had read a great deal about it, and had no small confidence in the recommendations of my authors. It could hardly be otherwise; every new cure was stated to be all but infallible, but whether it was that they were calculated especially for tropical countries and were unfit for this, or that cases written down as malignant cholera, were not *so* malignant as those met here, or from what other cause I cannot say, but I met more disappointment in one day than I expected to have encountered in a year. Before three days my confidence in opium was nearly gone; not that I found it inert, but fearfully the reverse: when brandy, capsicum, the warm essential oils, &c., appeared to produce no more exciting effect on some of the cases we had in the close of April last, than if they had been thrown into a vessel by the patient's bed side, opium in large doses quickly brought on the symptoms attending the poisonous action of this drug, and it was mostly only in large doses it manifested any action. When exhibited in the very cold and blue cases, where the pulse at the wrist was either not at all or hardly to be felt, in doses repeated every half hour or

every hour, its effects as a poison came on very insidiously ; and when the disposition to doze immediately after being roused by the attendants, by vomiting or such like, when the patient snored, or when the eye put on the appearance and character of one who had taken rather much of the medicine, and that its use was then suspended, its deleterious effects showed no disposition to subside, nor could the best efforts, even in a miserable minority of instances, stay the patient's progress into the state of apoplectic stertor and final fatality. I have given 20 drops (sometimes more) of tinct. opii every quarter of an hour, every half hour, or every hour, with a stimulating mixture composed of tincture of capsicum, brandy, and infusion of mint ; I have sometimes began in certain cases with 50, 60, or 70 drops, and continued it in smaller doses at intervals ; I have given it in every way I found it recommended in maximum and minimum doses, and while I am unable to find a single instance in my case book which I could call a recovery through its aid, I could point to many, during the short time I employed it, where it did irreparable mischief. Among the strongest prejudices I carried from authorities into the hospital was that against cold water. One of the India reporters, I recollect, states, that he never knew a patient recover who was allowed cold water to drink ; and other writers denounced it, although not so emphatically ; I accordingly requested the nurses not, on any account, to comply with the entreaties of the patients for cold water. On the 2d of May a female named *Margaret Insky*, aged 21, was admitted into one of my wards at 4 o'clock P. M., she had been nine hours ill ; the surface of her body was quite cold ; her feet, legs, hands, eyelids, and nose, were blue ; no pulse could be felt at her wrist, vomiting incessant of the rice water kind ; she had had two or three alvine dejections before admission, eyelids half closed, and eyes turned upwards ; thirst very great, and calls for cold water urgent ; cramps very distressing ; tongue cold and white ; pain just below the sternum, which she attributed to her having drank two pennyworth of

buttermilk during the morning. The necessary measures were resorted to for her : this girl having observed that a pail of water had been left near her bed by one of the deputies for some ward purposes, contrived unobserved during the evening to draw herself towards it, and putting her head into the vessel drank copiously ; it was speedily thrown up again, but the draught was repeated as often as she could without being detected. When I heard of it I was of course alarmed for the consequences ; however, during the night a patient in the same ward in a convalescent state, who had felt the deprivation of water herself not long before, got out of bed several times and supplied Insky with cold water ; this she told me herself at 6 o'clock the following morning, after I had expressed my satisfaction at finding her so much better than when I left her the night before. She recovered. This incident demonstrated, that the indulgence in drinking cold water was at least not certainly fatal ; I therefore commenced giving it in small quantities when called for, and soon after allowed them to drink as much of it as they pleased. I have found it the best drink of any I yet tried, and by far the most agreeable. There is no class of medicaments which has received so extensive a trial in cholera, or which through analogy appears more rational, than that of powerful stimulants. Brandy was reckoned indispensable ; the fame of cajeput oil was talked of by children ; it spread everywhere ; it was purchased by the heads of families, who hoarded it up lest the Moluccas might be exhausted, and the last hope of recovery be thus annihilated. An extensive dealer in drugs in France told me that the demand had been so great for cajeput oil that he did not think there was a pound of it at that moment unadulterated on the continent. I used for two months a variety of stimulants with various combinations, brandy, hollands, wine, ale, capsicum, camphor, the stimulating essential oils, ammonia, mustard, &c. They appeared to be inert in some ; others they seemed to rouse from the death-like state of collapse for a period, and in a great many they were decidedly prejudi-

cial from the beginning. If stimulants be used, I think it is a mistake to increase the potency of their excitement to the intensity of the collapse, or in other words to give the largest doses in the worst cases. The first time this struck me was in the instance of a man named Champion, admitted in May last; it was an extremely bad case; life had receded so far as to leave little hopes of recal; heat was applied, he was rubbed with mustard and vinegar, ten grains of calomel were given him, washed down by an ounce of the stimulating mixture mentioned above; the latter was repeated every twenty minutes for an hour and a half, at which time his look was rather less languid, he asked oftener for drink, and made observations on his condition; I increased the dose of the mixture by a third, but by mistake he got two doses with this increase almost at the same time, the nurse having given him one, and a deputy, not aware of this, gave another the moment after; I was engaged at the other end of the ward when I heard some wrangling about the mistake, and on coming over to the patient's bed I found a visible alteration in him for the worse; he became insensible, respired with intervals of some seconds, and gradually sunk; the medicine contained no opium. Patients in cholera whose cases seem to indicate the necessity of stimulants, almost uniformly express repugnance to swallow them after the first dose; confirmed dram drinkers will resist taking ardent spirits, either undiluted, or mixed in the usual way; I have repeatedly inquired after their taking half an ounce or an ounce of brandy, if they felt better or more comfortable, and the reply has almost always been "Oh no, sir;" nor will these things in general rest on the stomach. The disease having been partially suspended in the city, I ceased my attendance at the hospital on the 29th of May, but was recalled in almost a fortnight afterwards, in consequence of a sudden and rapid increase in the number of cases admitted; I found a house mixture extensively employed, of which I give the form.

℞. Aquæ carbonatis ammoniæ ℥ss.

Infusi menthæ comp. iv.

Syrupi Zingiberis ℥i. ℥.

Of this mixture an ounce was given every half hour, as the urgency of the case seemed to demand, sometimes with a calomel powder containing three or four grains, but oftener without this last; from the circumstance of finding the peculiar fluid ejected from the stomach in true cholera, on two or three occasions, possessing acid properties, and from the accounts I heard of the success attending its use, I commenced ordering it with some confidence in collapsed cases, generally with from three to ten grains of calomel. It certainly did not by any means answer the expectations I had formed of it. Fully aware that non-chemicals are not necessarily non-medicals, I thought it possible that a decomposition might have taken place between the calomel and ammonia detrimental to both as active agents, I gave them with as long an interval between each as the frequency of repetition would permit. Notwithstanding that I observed there was nothing administered, which, after the first or second dose, the patients disliked so much, I might say dreaded, yet I persevered for a long time, until I found it possessed no occult virtue, it was merely a powerful stimulant, and like the rest of that class, I had satisfied myself it oftener depressed the pulse than raised it: more frequently increased the languor and lassitude of the patient than roused him from his perilous stillness. I cannot account for this; there were some of those who praised its efficacy, to whose judgment I should most willingly pay every deference, and the respect I entertained for their knowledge of the malady induced me to persevere in the use of the ammonia mixture much longer than I should have done, had its exhibition originated with myself. I state the matter as I found it, and opportunities may unfortunately recur for the profession to decide on the utility of this and other stimulants in cholera. It was not the practice to give ammonia to children, and they were among the best cures in the hospital. Their use made me ac-

quainted with this fact in the prognosis, that the pulse may be restored for a while without any alleviation of the patient's distress or improvement in his situation ; I have known the pulse become sensible in a previously pulseless wrist under the steady use of stimulants, again decline, and so three or four times during the treatment, without any assignable cause, at least for its recession, and the case ultimately prove fatal ; in those cases, however, it is rapid and feeble, compressible with the slightest force, and sometimes irregularly intermittent. Whenever the other symptoms do not improve *passibus æquis* with the pulse, I do not hope much from the latter.

Opiates, carminatives, effervescing mixture, and other medicines, have been given to stop the vomiting ; I have tried every thing usually employed to tranquilize the stomach ; sometimes one thing would seem to have succeeded, and sometimes another, but each would in its turn fail in several other trials ; perhaps a strong infusion of coffee, with or without sugar, may be found the oftenest successful, but it must be made with care ; I have found the greatest difference between the *infusion* and *decoction* in favour of the former ; I was informed in September last, by a medical friend who superintended a large cholera hospital in Dublin, that he succeeded in one case in immediately checking the vomiting by the administration of 20 grains of nitrate of silver ; whether it was tried afterwards, I cannot say. I have not for some time sought to check the vomiting, but on the contrary have rejoiced to see a patient in cholera vomiting freely, the which if he did not I rather used means to induce this action ; the very worst cases, the most difficult to save, are those where the stomach has rejected its contents but once or twice within the first three or four hours ; such a case is likely to run rapidly into that listless, half lifeless state, not inappropriately denominated by some cholera asphyxia ; these alone are the cases where I have found emetics manifestly serviceable, when they can be got to act, but which unhappily is not always the case, at least to the desired extent. The emetic employed

in the hospital was mustard, of which two drachms of the powder were given in water, and repeated in ten minutes if necessary ; besides the quickness with which this substance usually acts, it was thought peculiarly adapted to these cases from its stimulating quality ; I have known four doses fail to cause vomiting, and in one such case I gave half a drachm of the sulphate of zinc, which operated energetically in about half a minute ; but I have known some administer the mustard emetic in all cases of low cholera indiscriminately, whether the patient was vomiting constantly from the disease or not. I do not see the rationale of this. In the cases to which I should confine the employment of emetics I think they serve in two ways, by the muscular exertion demanded in the act, and by unloading the stomach of its morbid contents, which, as I stated on a former occasion, the patient always wishes. With respect to the purging, it has been said greatly to weaken the patient, and to produce that lentor of the blood described by chemical investigators ; if this be so (and I confess I have not always found the degree of debility, and quantity purged, in a direct and reciprocal ratio to each other) it would be of great importance to check it at once ; I have seen various kinds of enemata injected, and draughts as various swallowed with this intent, without effecting any thing ; extracts, gums, absorbents, opiates, all failed. There is but one kind of purging which I have been really anxious to stop, namely, that in which almost pure blood is discharged, and which I believe is seldom observed but during the stimulant plan of treatment ; I had one case with this in which I gave no stimulants, but I suspect brandy or whiskey was given him before his admission : the most efficacious treatment I found was one or two drachms of oil of turpentine every hour or every two hours, according to the urgency of the symptom. Those who had had profuse rice-water purging and recovered, did not manifest more debility than those who had perhaps but two or three evacuations of this description during the whole period of their illness. To raise the temperature of the body by means

of very warm clothing and artificial heat, has been considered a step of the first consequence ; I believe it generally injurious, and never useful except to the feet. We had in the hospital a number of spirit furnaces well contrived for the intended effect ; they contained five burners, the number of which lighted, determined the degree of heat applied with sufficient accuracy ; and by means of iron bed cradles and the ordinary bed covering, the heat could be thrown evenly over the surface, the patient's head being excluded. There are many diseases, I imagine, in which this apparatus would be found of great utility. What are its effects in cholera ? A patient is brought in, the surface of whose whole body is as cold, literally, as that of a corpse in winter : he is enveloped in a long thick flannel shirt, put to bed, and two pairs of double blankets are put over him ; in a few minutes he is found pushing off the bed clothes—the nurse observes it, and they are speedily replaced and his hands put under them : the next moment they are found thrown to a greater distance, and the tapes that confined the bosom of his flannel shirt untied, or impatiently snapped across, and the whole surface of his chest bared ; he earnestly begs that he may not be covered up ; he is not heeded, and the nurse again proceeds to replace the coverings ; he cries out that he is burning with the heat, and cannot bear them : the spirit apparatus is now arranged and lighted ; he feels its influence, and his distress becomes intolerable ; how often have I heard them exclaim at such a moment, “ Oh, for God's sake take away the heat, I'm smothering, I can't bear it, let me die ! ” in a few minutes he ceases to complain, his head lies quietly on his pillow, but his respiration has become laboured ; the pulse in the neck is found weaker, his countenance looks more cadaverous ; the trunk is found of course heated, but covered with a gummy kind of sweat ; he no longer calls for drink ; the heater is removed ; his calmness remains, but it is that of a gradual and tranquil dissolution. Not having detected any advantage from external heat, after a long and anx-

ious trial, I discontinued it, and after a little time I went farther, and suffered patients to expose themselves as much as they pleased, satisfied that the warmth of the ward was sufficient; as the heat naturally returns to the surface, the patient feels the internal heat diminish, and by degrees he becomes almost as anxious to keep himself covered as he was before to strip himself. I have met cases in which the patient appeared indifferent to the application of external heat; they were in general either moribund cases, or those which had not a semblance of demand for it at all. Heat to the feet, by means of flannel bags of hot salt or sand, is often serviceable to relieve cramps, but hand frictions and compression over the swollen and firm muscles of the calf of the leg, or those of the fore-arm, answer I think much better; if heat be applied to the hands or feet it must be moderate, for the sensibility of the skin is so much exalted in the collapse of cholera, that what would seem but a pleasant warmth to the hand of a by-stander, appears burning to the patient. Stimulating applications were much used in the beginning; a mixture of flowers of mustard and vinegar, the consistence of molasses, used to be rubbed in very bad cases, with a piece of flannel on the legs for quarter of an hour or twenty minutes with considerable force; it had the effect of changing the blue colour to a reddish brown hue; it is difficult to say if these frictions were of real service; I did not perceive any, and discontinued them without apparent disadvantage. Sinapisms have been applied to the feet or epigastrium, and I think often without any specific intention, and perhaps sometimes without proper discrimination; I have found a sinapism over the region of the stomach often of great service in removing the burning heat in or about that organ, of which cholera patients frequently much complain, sometimes preceded by the application of a few leeches; it is however very painful after five or ten minutes; I do not know any other useful purpose to which it is available in cholera; I have known the patients flapping the bed-clothes for two days after its being

applied, to cool the part, although little or no trace of its operation could be observed on inspection. I should have mentioned before, that I sometimes found bottled ale rest on the stomach, and the patient call even for a third draught of it, but it generally made them dull and inclined to a heavy kind of sleep, which was not at all refreshing. I have known bottled eider given; it was not liked often, and did neither good nor harm.

I have given above an abstract of the practice in cholera, which I tried or saw others try; if I have omitted any thing of importance, the very short time I have had to arrange it must be my apology. I shall now briefly detail the mode of treatment I have for some time put in practice, and which I have found superior to any other by many degrees. I do not boast of having found that medical *chimera* called a *specific*, and what is more extraordinary in those times, I do not believe it one, or wish to lead others into such a belief, but I would urge a trial of it on the profession, as experience, laboriously purchased, has given me great confidence in its value. About the middle of July cholera had arrived at its acme in Dublin; from the 10th to the 14th, inclusive, 615 cases were admitted into the Grange Gorman hospital according to the registry; my nurses and deputies were worn out with fatigue; and as I feared to trust their vigilance too far, I remained as much in the wards allotted to me as possible; for nearly five weeks I was seldom in bed before sunrise; this constant *surveillance* brought me more into the minuter details of the cases, and insured the regular administration of the medicines. On the 13th of this month, after watching for several hours some bad cases under the stimulant treatment, I concluded my observations in my note book with these words, 'perhaps the disease should be treated more mildly—are internal stimulants good!' it was not the commencement of doubt, but the hesitation to make an experiment. I shortly after, began the treatment of cholera with Calomel, to the total exclusion of all stimulants; the form in

which I gave it was mixed with about a third of white sugar, put dry on the tongue, and washed down with a *small* quantity of cold water, for a good drink might cause vomiting, which of course immediately after the medicine would not be desirable. The dose I began with was ten grains every hour, or five grains every half hour, but I soon found this insufficient, and as I proceeded in the plan, finding there was no cause to fear present or remote injury from it, I ventured ten, fifteen, and even twenty grains every hour or half hour, as the urgency of the case demanded. The three first collapsed cases in which I tried these large doses, took these several quantities before they were sufficiently recovered to suspend its use; James M'Entee 440 grains of calomel in 68 hours; Mary Byrne 360 grains in 84 hours; Mary Burke 276 grains in 13 hours; Jane Brady, a delicate girl who had suffered all the privations of poverty, took 428 grains in 28 hours; I give these as examples of cold, pulseless cases, with copious rice-water vomiting and purging, and blue extremities. My limits will not permit me to give cases in detail, but I have noted thirty other cases of a similar description equally successful. There is but one kind of case which I have hitherto found to resist calomel, and that is one where nothing to my knowledge can be relied on for success; the patient is struck suddenly with complete prostration of strength, he lies as if asleep, his stools are involuntary, and *there is no vomiting*; he is deadly cold and without pulse; his skin is generally blue and clammy, but many are pale, and in some the skin is dry; they are rapid in their course. This is the kind of case in which I have repeatedly tried powerful emetics often without success; in one I succeeded in exciting vomiting which produced an astonishingly good effect, but a glass of brandy with cinnamon water was given him sometime afterwards, and he immediately relapsed into his former state, and soon sunk. From these large quantities of calomel in a limited time I have never seen mercurial eruptions, erythismus, dysentery, profuse salivation, or any one of the many other ill consequences resulting occasionally from

mercury. The gums are often barely touched ; sometimes there is a slight flow of saliva for two or three days ; the soreness of the mouth is generally felt in from 40 to 70 hours after the first dose, and in a week there is seldom a trace of it remaining. When reaction is established, the pulse has sometimes become too rapid and full, and the countenance rather flushed ; a single active cathartic removes these, and then the mouth becomes affected. I have had five cases where a constant and troublesome hiccough came on, when the pulse and heat of skin returned after the collapsed state. I tried with the first of these, a mixture of rhubarb and magnesia, then a blister over the stomach, and several other things, without the slightest benefit ; 20 drops of diluted sulphuric acid in a cup of water, every hour, succeeded in it and all the subsequent cases, in from the second to the fifth dose. The first stool, after that of the rice-water kind has ceased, is generally (in the calomel treatment) of a greyish colour and without fœtor ; the second, or at farthest the third, discovers bile. I allow the patients their favourite drink cold water, but as they improve and relish it less, I substitute often cold weak beef tea, carefully deprived of every particle of floating fat. The first flow of urine appears generally with the first or second grey stool, [could oxide of mercury give this hue ?] Under this treatment every symptom I have noted of cholera disappears ; the least of fever or debility has not followed in a single case I thus treated. The recovery is perfect in from two to four days.

I am aware of many of the imperfections in this sketch of cholera ; among the causes for these are want of space and time. I have used as few words as I could to explain my meaning, and in the pursuit of a practical fact, I have not stopped to round my periods.

ART. II.—*Observations on the Motions and Sounds of the Heart.* By DAVID C. NAGLE, A. B., M. B.; one of the Physicians to the Dublin General Dispensary, and, lately, one of the Senior Resident Physicians to the Townsend-street Cholera Hospital.

As the publication of facts, for the accuracy of which we can answer, is at all times advantageous to the cause of science,* the following case of heart disease is submitted for the consideration of the physiologist and medical practitioner. To those, who may have paid any attention to the two theories now before the profession, on the motions and sounds of the heart, it will not, it is hoped, appear altogether destitute of interest; and from the circumstances under which the case was examined, both before and after the occurrence of death, many medical persons having been present on both occasions, there could not, perhaps, be adduced one more unequivocally entitled to the consideration of these, whose opinions may as yet remain balanced between the two doctrines.

CASE.—David Connell, a servant, aged 18, affected with ascites, and anasarca of the lower extremities and face, was admitted into Sir Patrick Dunn's hospital on the 16th of November, 1831. Requested by some of the pupils to examine the patient's heart, I found him labouring under a distressing dyspnoea, with a hard dry cough aggravated on the slightest motion. When agitated or suddenly raised into a sitting posture, he suffered much from violent palpitations of the heart. His face was pale, but the expression of countenance calm and not indicative of anxiety: his respirations averaged 36, and the pulse varied between 80 and 100. The palpitations commenced, as I was informed, about two years previous to his admission, and in consequence of a fright; and for some time there was

* Andral, *precis d'Anatomic Pathologique*.

felt in the region of the heart a pain, from which he experienced much relief by the use of medicine. On applying the stethoscope to the region of the heart, I was astonished at the extraordinary energy of the impulse, and the loudness of the bruit de soufflet. The former impelled the cylinder against the ear with a force rapid, unusually energetic, and synchronous with the *commencement* of the soufflet and the arterial pulse at the wrist and temple. The cylinder, laid upon the spot, was observed to be elevated very sensibly at the instant of the beat in the arteries. The impulse at the chest was thus perceived by the ear and eye to be synchronous with the arterial pulse! The bellows sound was louder than any I ever witnessed, even in cases of utero-gestation: it was audible not only over every part of the chest, anteriorly, posteriorly, and in the axillary regions, but over the entire epigastrium: it was also occasionally remarkably prolonged. From its great importance in the diagnosis, I traced it with peculiar care to the point of loudest intensity, which I found to be nearly at the fourth rib of each side anteriorly. This, then, I naturally considered the centre of radiation. The soufflet entirely usurped the place of the first sound, was prolonged to the second, which it completely masked, and by which it seemed to be abruptly terminated; and then not a vestige of it or any other sound could be heard by me and others, between the second and first sounds of the heart. The soufflet was perfectly synchronous with the arterial pulsation and cardiac impulse! The rhythm of the heart's actions was frequently interrupted by a very prolonged intermission between the second and first sound, or, according to the old theory, between the contractions of the auricles and ventricles. Towards the termination of that intermission, the ventricle was perceived by the ear to rise slowly to the parietes of the chest; then expanding, as it were in preparation for a more violent effort to contract effectually, its systole, the soufflet and the arterial pulsation, all, all commenced at the very same instant, and with augmented energy. For this important phenomenon I

watched, and marked it with especial attention in consequence of its diagnostic value. When the patient was composed and in the recumbent posture, the palpitation ceased altogether, and the impulse was confined to a narrow space in the natural situation. There was no pulsation of the jugular veins; no lividity of the face; no hæmoptysis; nor impulse on the right side, or inferior part, of the sternum. Between the arterial pulsation and the impulse at the chest, I considered there was, in strength, a slightly unnatural disproportion; and the cardiac impulse conveyed the idea of its having been produced by a *rigid* substance. In the following diagnosis I conceived I was fully warranted by the leading symptoms now detailed.

DIAGNOSIS.

After a few minutes' examination I was asked by those who wished me to see the case, "if I was able to make any thing of it?" I replied in the affirmative, and that "I considered the lesion was chiefly confined to the left ventricle and aortic opening; that the ventricle was much hypertrophied, and that there was, at the mouth of the aorta, an obstruction to the free passage of the blood; that the auriculo-ventricular aperture was perfectly free, and the mitral valves unaffected by any material lesion." This diagnosis, formed in so short a time, astonished a few, who were prepared to find in the case a confirmation of the correctness of the new theory; to which, I had no hesitation in saying, "they would, upon a more minute examination, find the case directly opposed, as the result would testify." A second examination on a subsequent day, confirmed me in this belief, and strengthened my opinion, that the left ventricle was dilated as well as hypertrophied. The patient having died, rather suddenly, very soon after, I was supplied by Mr. A. Smith and my very experienced and intelligent friend, Dr. Nalty, with the following account of the post mortem examination of the heart.

AUTOPSY.

The heart was enormously enlarged, but the *entire* of the *right* side was in a *natural* state! the parietes of the left auricle were very slightly thickened, but the *left ventricle* was exceedingly hypertrophied and dilated. The left auriculo-ventricular orifice was *quite free*; the mitral valves presented a few opaque spots, but were fully capable of adequately discharging their office. The semilunar valves of the aorta were *very much hypertrophied*, and quite incompetent to the performance of their functions, being unable to close the aperture, and allowing water to pass freely from the aorta into the ventricle. The aorta was a good deal attenuated, and its calibre bore no natural proportion to the dilatation of the left ventricle, the former measuring but $2\frac{1}{8}$ inches, while the internal circumference of the ventricle measured eight inches! The external circumference of the broadest part of the heart measured $13\frac{3}{4}$ inches. The lungs and other viscera appeared healthy.

REMARKS.

In whatever rational way we view this case, the conviction inevitably forced upon our minds is, that, as far as one case can, it indisputably falsifies the new theory. In forming my diagnosis, I endeavoured to reconcile the symptoms with that theory; but soon perceived that the attempt would be a most unpardonable perversion of facts. For if we suppose the prolonged bellows-sound, which was synchronous with the impulse at the chest, to have been occasioned by the passage of the blood from the auricle into the dilated ventricle, we could *not* have the arterial pulse isochronous with it; yet all admitted their perfect synchronism. Here then is one fact that ought to be convincing against the validity of the new doctrine. Now let me suppose the soufflet to have been produced by regurgitation from the imperfectly closed aorta into the ventricle. On this supposi-

tion, and to entitle it to any value in sustaining the new views, it should necessarily have taken place *after* the arterial pulse and *second* cardiac sound ; but this was by no means the case, as Dr. Nalty, Mr. Smith and others are well aware. Again, if the soufflet originated in the descent of the blood from the auricle into the ventricle, its intensity ought to decrease in proportion as the ventricle was filled. Now, every minute observer must be aware, that this is not what happens in cases of hypertrophy of the left ventricle and obstructed aorta ; for the termination of the soufflet is, under such circumstances, generally as loud as its commencement, and this actually occurred in the case now adduced in support of the opposition I invariably felt myself authorized in giving to the new theory ever since the second or third lecture delivered on it.

When the soufflet is occasioned by an obstruction at the aortic orifice, as in the above case, it should, according to the new theory, commence *after*, and not synchronously with, the first cardiac sound and impulse at the chest, occupy but a small, and that the latter, portion of the interval between the first and second sound, and be terminated by the first. But my diagnosis was greatly influenced by carefully observing not only a perfect calm and total absence of the soufflet between the second and first sound, but that this soufflet was substituted for the first and was continued to the second ; that it occurred synchronously with the impulse and the arterial pulse. To all these points I even directed the attention of a few pupils of the hospital. If we next consider the state of the left auricle, we find in it no degree of hypertrophy to produce an impulse so extremely energetic as actually to raise, during the palpitations, the head of the auscultator, and jerk the cylinder off the chest. Besides, we know that the auricle during its contraction, descends under the ventricle, which is then dilating, and consequently in rather a flaccid state. In which condition it would, may I not justly remark, be physiologically incapable of communicating the *rigid* impulse, of which the ear is fully sensible in violent palpitations

of the heart. The ventricle's interposition, then, between the auricle and the parietes of the chest appears to me a fully adequate reason for disbelieving that the rigidity is effected by the contraction of an hypertrophied auricle. And, indeed, experience proves to us the existence of the most violent and distressing palpitations, without the slightest appearance of hypertrophy in either auricle. But to arguments, deduced from pathology, I have elsewhere* alluded, and need not now revert ; but I will take leave to add, that those who heard my diagnosis in the present case, and afterwards carefully attended to the inspection of the body, were pleased to express themselves in approbation of the views and opinions I have ventured to maintain.

There are a few other particulars, connected with the present case, not undeserving of attention, though they do not directly bear on the question under discussion. Authors are divided regarding the value of pulsation in the jugular veins, as indicating disease of the right side of the heart ; Corvisart and Testa supporting the negative, while on the side of the affirmative are arranged Bertin, Lancisi, and Laennec. Its existence, if I may hazard an opinion, is, at least, deserving of attention ; though when unsupported by other phenomena, physical as well as general, it can scarcely be deemed an unequivocal sign of a morbid lesion in the heart. In the case before us its absence, as well as that of the other phenomena, induced me to predict that the right side of the heart was free from disease : in the case I adduced, on a former occasion, to sustain my opinions relative to the new theory, I noted its presence, and there was disease of the right side of the heart. It is also to be remarked, that, in the case of Connell, there appears to have been a congenital contraction between the mouth of the aorta and the left subclavian artery ; which fact, if Corvisart's opinion be correct, is sufficient to account for the enormous dilatation of the left ventricle

* *Lancet*, 28th May, 1831.

in the heart of one so young. The absence of any lividity of the face appeared to me not unimportant in forming my diagnosis respecting the right side of the heart ; and perhaps in the contraction of the aorta we may find sufficient cause for the paleness exhibited by the countenance, notwithstanding the great hypertrophy of the left ventricle.

I would, lastly, beg leave to remark, that the knowledge arrived at by the employment of auscultation in the above case ought to mollify, at least, the hostility of *some*, who, of course, from their inexperience in its great utility, have recently volunteered such indiscreet and uncalled for denunciations against auscultation. If even in cases of heart disease, in which, all must admit, lesions are most difficult of recognition by the stethoscope even, its employment could lead to accuracy of diagnosis even one who does not pretend to any superior dexterity in its use, what may not reasonably be expected from its judicious employment by such as are furnished with better opportunities and keener powers of discrimination ? Surely it is not a legitimate mode of reasoning, to argue against its general utility from its failure in, perhaps, a few cases, or in the hands of those who may labour, perchance, under some physical incapacities, or, at least, may not be sufficiently experienced in the requisite tact. I hope there are in the profession but few who would thus sanction that unjust, sophistical, and irrational maxim “*Ex uno disce omnes.*” Such prejudiced, I would not say *interested*, persons, though they may do irreparable injury to those, whose education may unfortunately be confided to their care, cannot retard the advancement of this, to *them* unintelligible, improvement in medical science. How truly applicable to such persons are these anticipations of Laennec. “There is reason to believe, that long after the utility of mediate auscultation shall have been unanimously admitted by the better informed members of the profession, many practitioners will still be found to neglect or even disdain it ; who will, nevertheless, think their time not at all misspent in feeling the pulse of an hypochon-

driac, or examining, day after day, the fæcal excretions of a peripneumonic patient!"

ART. III.—*Contributions to the History of the Chlorine Salts of Barium.* By JOHN ALDRIDGE, Licentiate of the Apothecaries' Hall of Ireland.

HE who would write a history of the salts, should compose a chronicle of chemical science; their existence was recognized with the first application of human thought to the subject, and the knowledge of their properties has certainly been the most successfully cultivated branch of chemical investigation; yet we must not suppose, that chemists have always meant by the epithet "salt," a well defined class of natural compounds; it was probably at first confined to bodies of a crystalline texture, but gradually the term became applied to substances, the most dissimilar in their physical characters. We find it assigned at one time to an element, which, together with the imaginary principles, sulphur, spirit, phlegm, and caput mortuum, were assumed by the early chemists, as the ultimate constituents of all bodies on the surface of the globe. It is amusing to trace the characters ascribed to this hypothetical element, it was soluble in water, undecomposable by fire, and of a pungent smell; it was thought to be the cause of savour, in the same way as odour was ascribed to sulphur, colour to spirit; apparent differences in properties were all ascribed to accidental mixtures with the other elements. At a later period, even during part of the last century, the term "salt" had completely altered its definition; it was now considered that salts were mixtures of different substances with a pure elementary spirit, this last was considered as the true salt, the particles of which were represented to be composed of points. If we examine what Homberg and other

chemists of his day meant by this principle, we will find it to be in reality the different acids, and the most fanciful notions were indulged in to account for the diminution of their corrosive powers when combined: thus alkalies were supposed to be an aggregate of little sheaths, which receiving the pointed extremities of the saline principle, (the acids,) disarmed them of their energy; what were called the lixivious salts were imagined to be spongy masses of foetid oil, containing the element in their areolar tissue.

The extension of knowledge, the march of discovery, however, emancipated chemistry from these crude hypotheses; data furnished by continually accumulating facts, and minds enlarged by the investigation of mathematical science, enabled the philosophers of the last century to generalize, to limit, and to enlighten; the epithet "salt" was now confined to the combination of an acid with an alkali, an earth, or an oxide. All acids were supposed to be combinations of a base with oxygen, and although alkalies and earths were even by Lavoisier imagined to be nothing more, still certain attributes behaved as distinctions, sufficiently explicit for the time, and a beautiful, although, preternaturally refined, simplicity supplanted the chaotic irregularity, wild and scarcely definable speculations of the previous era.

This state of science was however but of short continuance, the discovery of what were erroneously called the hydracids, and the imaginary combinations of these substances with the metallic oxides, produced considerable confusion in the above arrangement, while the increased knowledge consequent on the discovery of chlorine, bromine, cyanogen, &c., and the marked resemblance between these principles, and the great generator of acidity, as oxygen was long considered, prepared the minds of chemists to receive the doctrines of Berzelius, whose classification it must be acknowledged was still far from perfection, as it severed compounds so very analogous as the oxides and haloid salts. But still his happy view of the com-

position of the sulpho-salts, and his arranging them under the head of amphide salts with the oxy-combinations, pointed out the strict analogy between these compounds, and was certainly a grand step towards the elucidation of their nature.

Later writers have improved on this idea, Von Bonsdorff has included in the amphide salts, the chlorine iodine cyanogen, &c. combinations, and the many excellent arguments, lately brought forward, in proof of the positive energy of hydrogen, bid fair to banish for ever the anomalous muriates, hydriodates, and hydrocyanates. The chemistry of the salts, it may be seen, is advancing rapidly, our knowledge of their probable existence has even outstepped the march of experiment, and it will require years of labour and research to fill up the outline which the theorist has conceived. But if our knowledge of the constitution of individual compounds has become more accurate, we must still acknowledge, that the term by which these species have been linked, is totally inadequate. We have found it represent successively certain imaginary properties, and a state of combination; the first ground of classification is objectionable in a science like chemistry, even if the properties assumed were really existant; in fact, what do we understand by a saline taste, or a saline appearance? Not certainly the taste of chloride of hydrogen, or the appearance of sulpho-cyanuret of hydrogen; yet we cannot separate the one from chloride of sodium, or the other from sulpho-cyanuret of potassium. But if certain external characters are improper as a ground of chemical classification, it is equally undeniable that we cannot with any propriety represent, by this term, certain states of combination. Prostituting the term in this way would only add to the confusion, which the practice of confounding technicalities with words of ordinary and different meaning must always occasion.

It may appear misplaced to enter into a discussion on the subject of classification, in a paper devoted to a different object. But I thought it necessary to engage in some details on the present state of chemistry with respect to the salts, that the principles, on which the genus I am about to describe is found-

ed, might be fully appreciated by those readers who may not have exercised the constant vigilance, which is necessary to keep pace with the rapidity of discovery. The time is probably approaching when the term "salt," invented in an age of ignorance, changing its proteus meaning with every new discovery, and applied to compounds the most distinct, nay, opposed in their attributes, will be entirely discarded from a science that is rapidly acquiring strictness and certainty.

I have already said that the views which late chemists have entertained of the composition of the amphide salts, has opened to the experimental chemist a mine of rich promise for research. The philosopher who wishes to enlarge the boundaries of science, may now see whole families of combinations, hitherto unexplored, awaiting his patient investigation, and indeed it is a duty which he who loves the science owes to its advance, and a privilege which he should so appreciate, to examine the surmised compounds, and thus form an extensive superstructure of facts, on which future discoveries and future speculations may be erected. The chemical adventurer has here a reasonable and certain foundation for experimental inquiry. These reasons have induced me to undertake the examination of some of these compounds. Perhaps it may be necessary to premise with a definition of chlorine salt: by this term is meant a substance composed of two primary compounds, in each of which chlorine is the electro-negative element. In fact, as an oxy-salt is composed of an ox-acid, or a primary compound with oxygen in which the electro-negative energy predominates, and an oxy-base or one which is positively excited; so a chloro-salt is precisely analogous, only the oxygen is in the latter replaced by chlorine. Some of these compounds have been long known, for example, the chloro-hydrargyrate of ammonium, (sal alembroth), and others of the chloro-hydrargyrates have been discovered by Bonsdorff, the author who first recognized the true nature of these combinations, which were previously considered as double chlorides, but the superior simplicity and elegance of

his hypothesis must soon command universal assent. I am not aware that any of the chloro-salts of barium, have been hitherto examined, and I have therefore selected them for investigation.

DI-CHLORO-ANTIMONATE OF BARIUM.

By digesting a solution of chloride of Barium, with a solution of chloro-antimonate of hydrogen,* I procured a lemon yellow precipitate insoluble in excess of chloride of hydrogen; I diffused this precipitate, which weighed 30 grains, through distilled water, passed a stream of sulphuretted hydrogen through it, and thus threw down the antimony; but aware of the difficulty of quantitatively estimating this metal, I proposed determining the barium and chlorine, and calculating the quantity of antimony from the loss. I therefore boiled the supernatant liquor for the purpose of getting rid of any excess of sulphuretted hydrogen, filtered and added an excess of dilute sulphuric acid; having allowed the sulphate of baryta to subside, and filtered off the liquor, I cautiously added nitrate of baryta as long as any turbidness was produced, to get rid of the excess of sulphuric acid, and then acidulating the solution a little with nitric acid, and adding an excess of nitrate of silver, I permitted the chloride of silver to separate, and filtered off the clear liquor. Both precipitates were then dried, ignited, and weighed. The sulphate of baryta weighed 20.5 grains. The chloride of silver 45.75 grains. Now by calculating the quantity of barium and chlorine in these compounds, and subtracting their united sums from the weight of the salt subjected to analysis, we can see at once the proportion of antimony; 21.9 grains of sulphate of baryta contains 12.87 grains of metallic barium, and 47.725 grains, which is

* Although this salt has never been insulated, we yet may infer its existence from the fact of chloride of antimony being soluble in an aqueous solution of chloride of hydrogen, (muriatic acid)

very near the quantity of chloride of silver obtained by experiment, represents 11.025 grains of chlorine ; but we are to recollect that the barium is supposed to exist in the compound as chloride, and thus 6.63 grains of chlorine are provided for, and we may set down the proportion of chloride of barium to be 19.5 grains. But 30 grains was the quantity at first operated on, from which if we subtract 19.5 we will get a remainder of 10.5 grains, consisting of 4.395 grains of chlorine and 6.105 of antimony ; the precipitate is thus shewn to be compound of 19.5 grains of chloride of barium and 10.5 grains of chloride of antimony, but the atomic equivalent of chloride of barium is 104.15 ; let us therefore state it thus, as 19.5 is to 30, so is 104.15 to 160, and a decimal. Now by subtracting 104.15 from this, we get the quantity of sesqui-chloride of antimony, the atom of which 117.776. But here we only get the half of that, so that we infer this precipitate to be composed of

2 ch. bar. + 1 ch. ant.

BI-CHLORO-ANTIMONIATE OF BARIUM.

The supernatant liquor, after the precipitation of the above was evaporated, and when refrigerated, produced a crop of brilliant stellated crystals, these were found to be decomposed by water, and some oxide of antimony thrown down. I diffused 40 grains through distilled water, and analyzed it in the same manner as the preceding ; 67.5 grains of chloride of silver and 13 grains of sulphate of baryta were thus procured : 68.446 grains of chloride of silver contains by calculation 16.686 grains of chlorine, and 13.72 grains of sulphate of baryta, 8.08 grains of barium ; now 8.08 grains of barium require 4.174 grains of chlorine to form chloride of barium, and thus we find 40 grains, the quantity subjected to examination, to contain about 12.254 grains of chloride of barium. To find the quantity of chloride of antimony we have only to subtract 12.254 grains from the whole quantity, which gives 27.746, composed of 12.512 grains,

being the remaining chlorine, and 15.034 loss, which of course represents the antimony ; we may then represent this compound as follows :

ch. a. 27.746 or 2 atoms chl. ant. 235.75

ch. b. 12.254 or 1 atom chl. bar. 104.15

DI-CHLORO-ANTIMONIATE OF BARIUM.

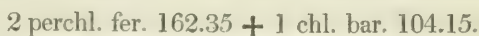
Some of the crystals described above were decomposed by distilled water, and the solution cleared from the oxide of antimony and evaporated, yielded flaky crystals of a dull white colour ; these were perfectly soluble, precipitated white, with carbonate, and yellow prussiate of potash, and became turbid with fluosilicate of potassium ; 38 grains were dissolved and analyzed like the former combinations ; from the above quantity I obtained 60 grains of chloride of silver, and 18.5 grains of sulphate of baryta. Now 60.60 grains of chloride of silver is composed of 14.77 grains of chlorine and 45.83 grains of metal, so by this means we know the quantity of the electro-negative element, and 19.45, which, allowing for some inaccuracy of manipulation, we may assume as the proportion of sulphate of baryta, contains 11.28 grains of barium, 6.08 grains of chlorine are appropriated to 11.28 of barium, and thus we have 9.169 grains of chlorine to unite with the antimony which the difference represents.

$$\begin{aligned} \left. \begin{array}{l} \text{ch. } 9.169 \\ \text{ant. } 10.760 \end{array} \right\} &= 19.929 \text{ ch. ant.} + 17.36 \text{ ch. bar.} \left\{ \begin{array}{l} 6.08 \text{ chl.} \\ 11.28 \text{ bar.} \end{array} \right. \\ &= 1 \text{ atom chl. ant. } 117.775 + 1 \text{ atom chl. bar. } 104.15. \end{aligned}$$

CHLORO-FERRATE OF BARIUM.

When you add a boiling and nearly saturated solution of chloride of barium to a solution of perchloride of iron, a considerable crystalline precipitate is deposited, and if you evaporate the mixed solutions you may procure much more of the same compound by crystallization. The two products which are the

same in composition may be dissolved in distilled water, and by the usual process, crystals, with forms apparently derived from the oblique rhombic prism, can be procured ; these, when dissolved in distilled water, produce a blue with yellow prussiate of potash, a white precipitate with phosphate of soda. I analyzed them by boiling with dilute sulphuric acid, and after removing the sulphate of baryta, saturated the remaining acid with ammonia, and then adding an excess of the same precipitant, I boiled the liquor, and procured peroxide of iron by filtration ; the filter was burned and the peroxide ignited : the quantity operated on was 40 grains ; 17 grains of sulphate of baryta and 12 grains of peroxide of iron, were obtained by the above process. Now even allowing for the ashes of the filter, the precipitated peroxide may be estimated at 12.307 grains, which will give us an equivalent of about 24 grains of the perchloride, leaving 16 grains as the proportion of chloride of barium, which comes very near the amount calculated from the quantity of sulphate found by experiment ; now 16 bears to 104.15 (the atom of chloride of barium) the same proportion as 24 does to 162.35, but this is twice the equivalent of an atom of perchloride of iron ; therefore the crystals of chloro-ferrate of barium appear to consist of



CHLORO-CUPRATE OF BARIUM.

A solution of neutral perchloride of copper poured into a concentrated solution of chloride of barium, throws down a white granular precipitate, the quantity of which may be increased by heating the mixed liquors ; upon separating the precipitate and evaporating the supernatant solution, crystals will form in six-sided tables, and which prove, when tested, to be nearly pure chloride of barium : 20 grains of the precipitate, however, on being boiled with an excess of carbonate of ammonia, gave 8 grains of carbonate of baryta, and the supernatant liquor became

darkly blue ; now 8 of carbonate is equal to about 8.5 grains of chloride, which would give us a difference with the quantity operated on 11.5 ; and if we suppose the precipitate to be composed of an atom of each constituent, the calculated proportion of chloride of copper (that is supposing it to be composed of 2 ch. + 1 cop.) would be 11.08, which comes so near the above amount of difference, that its coincidence is really surprising ; we therefore may conclude that this precipitate is a definite compound, at the same time we cannot place much confidence in the surmised proportions of its constitutions, and I have not had time to institute a more accurate estimation.

CHLORO-MANGANESATE OF BARIUM.

Upon boiling proto-chloride of manganese with chloride of barium, and evaporating, I procured a mass of irregular and very minute crystals ; these if quite pure would be probably perfectly white, but the proto-chloride containing a trace of iron gave them a brownish tinge ; the same cause obscured the action of re-agents, but by passing sulphuretted hydrogen through the liquor and boiling for a few seconds, I was enabled to observe a pinkish white precipitate on the addition of yellow prussiate of potash ; this however soon became obscured by exposure to the air ; fused with soda also they gave their characteristic green. I boiled 40 grains of the salt with dilute sulphuric acid, and after the separation of the sulphate of baryta, the remainder was evaporated to dryness, and the sulphate of manganese ignited ; the sulphate of baryta weighed 28.5 grains, and the sulphate of manganese 17.5 grains ; now 29.175 grains of sulphate of baryta contains 17.175 grains of barium, which is equal to 25.54 of chloride of barium, and there ought to be 15.46 grains of chloride of manganese, if the salt is, as I suspect, composed of an atom of each constituent. This would contain 7 grains of manganese, equivalent to 19 grains of the sulphate. But as I only obtained, as I stated before, 17.5 grains, I rather ascribe the

deficiency to some error in manipulation, than to the incorrect supposition of its constitution.

CHLORO-HYDRARGYRATE OF BARIUM.

This salt is difficultly crystallizable and extremely deliquescent; it precipitates white with ammonia, white at first, afterwards black, on repeated additions of sulphuretted hydrogen water. It may be prepared by boiling together corrosive sublimate and chloride of barium in the atomic proportions, and subsequently evaporating to dryness; it has I find been previously examined by Boullay.

To detail the properties of all the chloro-salts of barium would lengthen this paper beyond the limits of a periodical; I will therefore conclude with a hope that others of my fellow-students will enter on this field; in it they will find novelty to delight, facts to satisfy, and exercise for their analytic resources. There may be but little honour to be gained by the examination of compounds, the surmise of whose existence must necessarily result from natural laws previously discovered; yet it surely must afford some pleasure to connect ourself even by a common object of pursuit with the lofty mind that first conceived them; that mind which equally capable of the most magnificent conception, and the most minute detail, has elevated chemistry, among the sciences, to a rank as proud as any which ever courted the philosopher, or recompensed the votary. I shall continue at a future period the further examination of these salts.

ART. IV.—*On Gangrene succeeding to Cholera.* By WILLIAM A. DAVIS, Surgeon, Newry.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL AND CHEMICAL SCIENCE.

“SIR,

“Should the annexed (to me) unusual termination of a case of cholera be considered worthy of a

space in your valuable Journal, your giving it insertion will oblige

“Your obedient Servant,

“WILLIAM ALEXANDER DAVIS.”

T. G., ætat. 50, a pensioner, upwards of six feet high, and well proportioned, has been under my care for five months past, labouring under dyspepsia, during which time his state of health varied with our reports of cholera, which he had a great dread of; when the report was favourable his health was better, and *vice versa*, proving the powerful effect the state of mind has on the chylopoietic viscera. On Wednesday, the 19th September, he was affected with diarrhœa, which continued little abated until Friday the 21st, when he had a regular attack of cholera; at 4 o'clock on Saturday morning (the 22nd) re-action took place, from which hour until 7 o'clock in the evening, he had no ejection or dejection, but was going on as well as could be wished, when having a desire to go to stool, he very imprudently (no person being in the room with him) got up, but had no motion; after lying down he was seized with severe spasms in his right leg, which continued for a few hours, rendered less by friction with warm flannels, and when they subsided he was deprived of warmth, all sense of feeling and motion half way down the leg; the next day, the 23rd, the leg in different parts had the appearance of ecchymosis, which I supposed might be caused by the friction, the want of heat and sensibility continued. 24th, this morning the toes were seized with gangrene, which gradually increased, and there was little constitutional disturbance until the 27th, when he had hiccough; pulse upwards of 100, and gangrene spreading; he died at 10 o'clock on Friday the 28th, gangrene having reached to within 4 inches of the knee; no line of separation had formed. A hurried post mortem examination of the *leg only* was allowed, but nothing particular was discovered, except that the posterior tibial artery was considered

smaller than it usually is. The case was seen by most of the respectable practitioners in the town and neighbourhood, to whom, as well as myself, it appeared novel, which is the reason I was induced to trouble you with it.

ART. V.—*Cases of Partial Fracture of the Long Bones in Children.* By JOHN GEORGE DALTON, Surgeon to the Baillieborough Dispensary.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL AND CHEMICAL SCIENCE.

“SIR,

“Should the following cases, illustrative of the success of the plan of treatment proposed by Mr. Hart in the first Number of your valuable Journal appear worthy of notice, you inserting these at the first opportunity, would oblige

“Yours,

“JOHN GEORGE DALTON.”

A BOY, 18 months old, fell into a sand pit, having his hand and forearm twisted under him; twenty minutes after which he was carried into the dispensary. The appearances then evident were, the hand and forearm marked with sand and dirt, and just at the middle of humerus on the inside, a firm angular protuberance existed; a corresponding depression evident on opposite side; no alteration in length of limb; very little pain produced by handling; no swelling; the child had the limb hanging by his side, but by an effort could raise it, and bend the elbow without assistance; the motions of elbow joint, and forearm, perfectly distinct and natural. Although I was perfectly well aware of the occasional occurrence of such an accident as a partial fracture and bending of the bone in a child,

I had never before met with it in practice, had I however been ignorant of its nature, Mr. Hart's paper so clearly described its appearance, &c., that a mistake in recognizing it could not occur. It struck me, however, that had it been situated nearer the elbow joint, it might have been confounded with a separation of the epiphysis from the shaft of the bone, the treatment of which, if not differing, would not, I should think, render it a mistake of much consequence. I made an assistant fix the upper part of the arm, whilst with a very gentle extension I cautiously straightened and pointed the bone into its natural form, (keeping in mind, the necessity of using a delicacy of manipulation, lest by completing the fracture I might seriously aggravate the injury): during which the child complained very little, and I easily and fully succeeded in my object, after which the limb appeared in no way to have suffered from the accident. The little fellow would have resisted as unnecessary the idea of confining the limb, which he appeared quite willing and able to use. I applied, as in ordinary fracture of this part, bandage, pads, and splints, for which latter I used pasteboard wetted and fitted for the limb, which I placed in a sling in the semiflexed position. I saw the child almost daily, very little pain or uneasiness occurred, on the 8th day I cautiously removed the bandage, &c., entirely, and I could not see any alteration in the strength of the limb, nor any deformity, except a very trifling hardness just at the spot where the bending inwards had existed. He applied the bandages, &c., and yesterday, fourteen days after the occurrence of the accident, I finally removed them, and the arm appears, with the exception of the slight hardness, (which is just perceptible on a minute examination), in every respect equal to the other in strength and appearance.

I was lately attending in a gentleman's family, in whose son, now about 3 years' old, there appeared when a few months' old, a prominence just at the sternal extremity of the clavicle, but how produced could not be ascertained: the efforts then used

could not remove it, and as it produced no apparent inconvenience to the child, the parents felt no alarm at its continuance. On a careful examination now, I can plainly feel the clavicle bent upwards suddenly, close to the sternal articulation, appearing as it were swollen above, hollow below; the joint is very lax. I did not propose any plan for its removal, not being aware of any which could now remedy this deformity, which however is not attended with the slightest impairment of power in the motions or strength of the arm.

For many months after its appearance, I understand, various bandages, compresses, &c., had been ineffectually used. It appears to me evidently to have been a partial fracture and bending of this portion of the clavicle, produced probably by a sudden force applied at the extremity of the bone at the shoulder joint.

ART. VI.—*On the Pathology of Dislocation of the Shoulder Joint.* By PHILIP CRAMPTON, M.D., F.R.S., Surgeon General to the Forces in Ireland, and Surgeon in Ordinary to the King.

THE *treatment* of dislocation of the shoulder joint has in all times engaged much of the attention of practical surgeons. The *pathology* of the affection, however, (on which alone a rational mode of treatment can be founded), has been but sparingly illustrated. In the year 1810, we find that excellent and experienced surgeon Mr. Hey of Leeds, lamenting that “the opportunities of dissecting the shoulder joint in a state of dislocation are so rare, that we still remain ignorant of the precise nature of the injury done to the several parts concerned in ordinary cases.” In fact, at the period when Mr. Hey wrote his “Observations on Surgery,” there was (I believe) but one case in record in which the actual state of the joints in a *recent*

dislocation of the shoulder joint was described and delineated ;* and even this case is deprived of much of its value, from the confused manner in which it is described, and from the very imperfect plate with which the description is accompanied ; neither was the case strictly speaking a *recent* one, as eighteen days had elapsed between the reception of the injury, and the examination of the joint after death ; and during this period the parts seem to have undergone considerable changes, both in their structure and mutual relations. The celebrated monograph of Professor Bonn,† contains (I should suppose) all the information respecting the pathology of dislocation of the shoulder which could be brought together up to the year 1782. He gives the anatomy of several cases of unreduced dislocation of the humerus, but the most recent is of two years' standing : these cases, therefore, leave the question as to the "precise state of the parts in *recent* dislocation," and the obstacles which are opposed to reduction, untouched. Such, I believe, was the imperfect state of our knowledge on this subject, when Sir Astley Cooper, whose talents and industry, aided by a vast experience, have illustrated so many important points of pathological, as well as practical surgery, published his account of the dissection of two cases of recent dislocation of the humerus.‡ To cases so universally known, it cannot be necessary to allude more particularly than to state, that both were instances of dislocation downwards or "into the axilla,"—that, in the first case, "the capsular ligament was torn on the whole length of the inner side of the glenoid cavity, which (rent) would have admitted of a much larger body than the head of the os humeri through the opening. The tendon of the subscapularis muscle was also extensively torn," but the tubercle on which the supra and in-

* Observations on a Dislocated Shoulder, &c., by Henry Thompson.—*Med. Obs. and Inquiries*, 1761.

† *Commentatio de Humero Luxato*.

‡ *Treatise on Dislocations*, 1824, page 375.

fra spinatus, and the teres minor muscles are inserted, was not (as in Mr. Thompson's case), torn off. In the second case in which dislocation had existed (unreduced) for five weeks, the capsular ligament had given way in the axilla between the teres minor and subscapularis muscles, the tendon of the subscapularis was torn, though, at its insertion, all the articular muscles, but particularly the supra-spinatus had been more or less lacerated, as it would seem, in the attempts which had been made at reduction. Sir A. Cooper found that "the resistance to reduction (even after death), was such as he could not by himself overcome, he divided one muscle after another, cutting through the coraco brachialis, teres, major and minor supra-spinatus muscles, but still the opposition to his efforts remained; he next divided the deltoid muscle, and found that the supra-spinatus muscle was his great opponent, *until he drew the arm directly upwards*, when the head of the bone glided into the glenoid cavity." To these interesting cases, illustrating as they do, so many important points connected with the management of dislocation of the humerus downward, or into the axilla, I am happy to have it in my power to add two additional ones, which have fallen under my own observation, one of a recent dislocation downwards, and one of a recent dislocation forwards under the pectoral muscle on the sternal side of the coracoid process.

CASE I.—In the year 1808, a labouring man was brought into the county of Dublin infirmary in a dying state, the persons who carried him stated that he had been engaged in digging under the foundation of a house that had been burned, that a part of a partition wall fell upon him, and that they had found him with his head buried under the rubbish. The man did not survive more than a couple of hours; on examining the body 18 hours after death, it was observed, that in addition to the injury of the head, which had proved fatal, the right humerus was dislocated into the axilla; to this part I directed the whole of my attention, assisted by my lamented friend and colleague

the late Mr. Dease, and in the presence of some of the other surgeons of the hospital, I made a careful dissection of the joint, previously to reducing the dislocation, and was so fortunate as to obtain a drawing of the parts, executed on the spot, by a distinguished artist.* On removing the integuments of the axilla, the cellular membrane, which was extensively ecchymosed, formed a kind of cap, closely embracing the head of the os humeri, which when the axilla was cleared, was seen lodged on the inferior costa of the scapula, or rather on its neck; the head of the bone in escaping from its socket had pushed the teres minor downwards, and burst through the lower part of the subscapularis muscle, some of the fibres of which closely embraced the neck of the bone, while the bulk of the muscle was pushed upwards and detached from the inner surface of the scapula.† The neck of the humerus, therefore, was in some degree embraced by the divided fibres of the subscapularis muscle, while a portion of its head rested on the neck and part of the venter of the scapula without the intervention of any muscular substance. The short head of the biceps and the coraco-brachialis were forced to describe a curve outwards over the neck of the humerus on the sternal side, while the long head of the triceps crossed the neck of the bone obliquely on the dorsal side; this strangulation of the head of the bone, by the surrounding muscles, was made most apparent when extension was applied to the fore-arm. The biceps and triceps seemed then to close behind the head of the bone, and interpose themselves between it and the glenoid cavity, the tendon of the long head of the biceps remained in its groove, but the sheath in which it runs was partially ripped up.

The capsular ligament was completely torn from the lower part of the neck of the humerus to the extent of more than half its circumference, the torn edge appearing like a crest over the head of the bone.‡ The great nerves and blood vessels of the

* The late Mr. Hamilton, the justly celebrated portrait painter.

† See Plate I.

‡ See Plate I. Fig. K.

arm were forced to describe a curve backwards, by the pressure of the head of the bone which was in contact with them. But the greatest injury had been sustained by the "*articular muscles*" (as they have been called) which lie on the back of the scapula. The tendons of the supra-spinatus, the infra-spinatus, and the teres minor, were completely torn off from the humerus, carrying with them, however, a scale of bone, which was ascertained to be the surface of the greater tubercle into which they were inserted.

In order to ascertain the nature of the obstacles which oppose the reduction of the dislocated humerus, the scapula was fixed, and the arm being raised to nearly a right angle with the body, extension was slowly applied to the arm by pulling at the wrist, it then appeared that so long as the hand was held *supine* the head of the bone remained immoveable, the chief resistance appearing to be caused by the closing of the biceps and triceps behind the head of the bone. The muscles on the back of the scapula being detached from the greater tubercle could of course afford no resistance, but on turning the hand into the *prone* position, and giving a motion of rotation inwards to the whole limb, the extension being still maintained, the head of the bone glided easily into its socket.

The appearances observed in this case are nearly identical with those which are described by Mr. Henry Thompson in the Medical Observations and Inquiries, while they differ materially from those which were found by Sir Astley Cooper, establishing an important fact, which indeed might have been inferred a priori, that in *apparently* similar dislocations of the humerus there may be very different *kinds* as well as *degrees* of lesion, and consequently very different causes of resistance to reduction.

In Mr. Thompson's case, as in mine, "the head of the bone was found lodged on the inside of the neck of the scapula, between the subscapularis and teres major muscles," but during the 18 days which had elapsed since the injury had been re-

ceived, the cellular substance of the axilla had formed a kind of capsular ligament "which embraced the head of the bone, and contained a small quantity of mucus resembling synovia."

In Mr. Thompson's case "the capsular ligament was completely torn from the whole circumference of the humerus," in mine it was detached to the extent of more than half the circumference. In both cases "the attachments of the tendons of the supra and infra-spinatus muscles were torn off with the part of the bone they were inserted into."*

In both cases "some fibres of the subscapularis muscle embraced the neck of the bone."†

In Sir Astley Cooper's cases, on the contrary, although the tendon of the subscapularis was torn through, the supra and infra spinatus muscles retained the connexion with the greater tubercle, and "*until this muscle was relaxed, by raising the arm, the humerus could not be reduced by any efforts which he (Sir Astley) could make.*"

The following is an instance of *primary* dislocation forwards, in which the head of the bone is thrown at once in the neck of the scapula, without previously passing into the axilla. I believe there is no dissection on record of a *recent* dislocation of this kind; there is, however, a specimen of one, which had been long dislocated, in the Museum of St. Thomas's Hospital.‡

CASE II.—James Wilson, aged about 30, fell into a lime-kiln in the immediate neighbourhood of the County of Dublin Infirmary, while the lime was still burning: he was drawn up by ropes, but just as he reached the top of the shaft the rope broke and he again fell to the bottom, a distance of about 15 feet, on the ignited stones; as soon as he was extricated from this dreadful situation, he was carried to the Meath Hospital, when he received from Mr. M'Namara, who happened to be on the spot, every relief of which his miserable situation was susceptible. It

* Med. Obs. and Inq., Vol. II., p. 349.

‡ Med. Obs. and Inq., *ibid.*

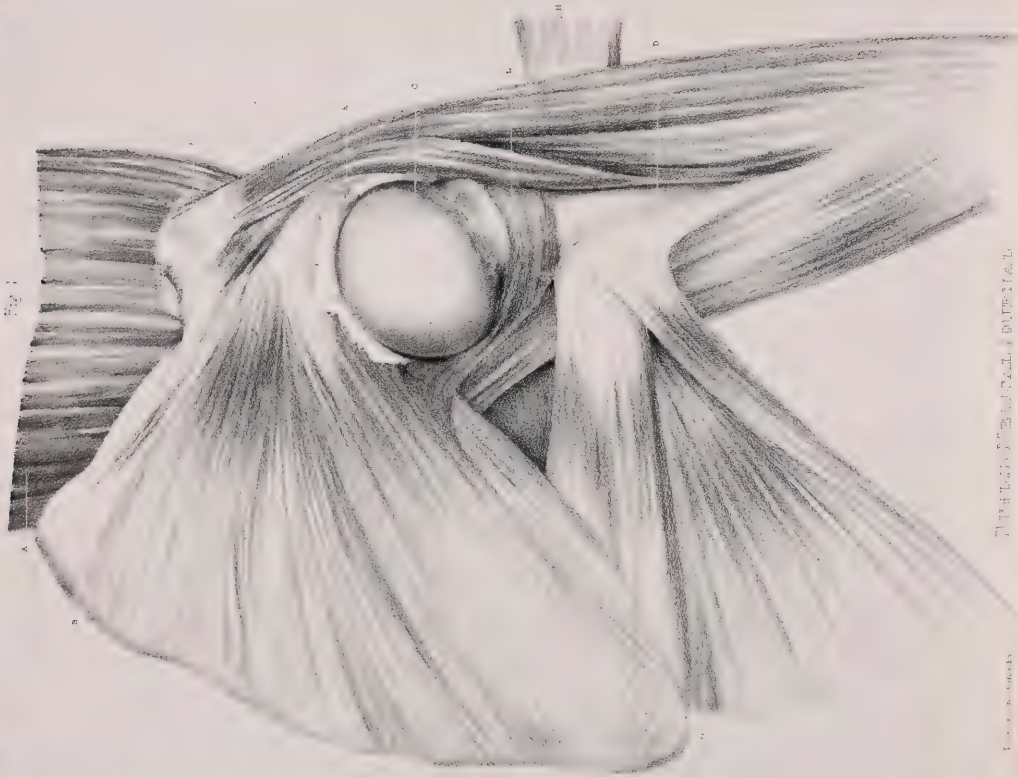
† Sir Astley Cooper's Treatise on Dislocation, p. 389.

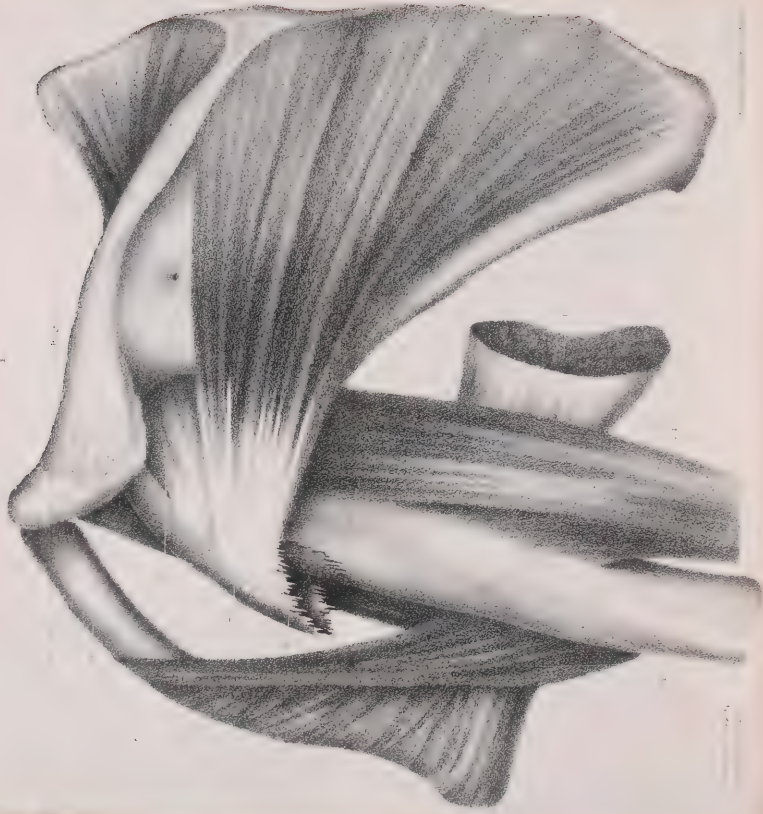
appeared, on examination, that in addition to several extensive burns and lacerations, that there was a dislocation of the humerus, under the pectoral muscle.

Mr. M^cNamara (without assistance), reduced the bone by merely drawing the arm gently forwards and downwards with one hand, while he pushed the head of the bone towards the glenoid cavity with the other. The poor man died in the course of the day, from the conjoint effects of the burn and the fall. Eighteen hours after death the shoulder joint was dissected by Mr. M^cNamara, from whom I take the description of the appearances, with the advantage of having the preparation before me while I write.

The dislocation was unattended with the rupture of any muscle, or the separation of any tendon from its insertion into the bone: by a slight effort the dislocation was reproduced, and the pectoral muscles being removed, the polished head of the bone was now seen lodged on the cervix of the scapula, at the root of the coracoid process, but extending nearly as far as the notch in the superior costa, it had passed out through a rent in the capsular ligament, *over the upper edge* of the tendon of the subscapularis, detaching this muscle from its connexion (which at this point is but slight) with the inner face of the scapula, and pushing its fibres downwards, so that they formed a curve which partly embraced the neck of the humerus; the supra and infra-spinatus muscles were on the stretch, but had suffered no injury. The cellular substance covering their tendons was deeply ecchymosed, so as to mark their course most distinctly. On replacing the head of the bone, the opening in the capsular ligament, through which it had escaped from its socket, could be distinctly seen. It was formed by a separation of the ligament from the interior side of the brim of the glenoid cavity from top to bottom; it was bounded at the top by the tendon of the supra-spinatus, and at the bottom by the inferior edge of the tendon of the subscapularis, the rent was continued as far as the root of the lesser tubercle of the os humeri, and







was of sufficient extent, but no more, to permit the head of the bone to pass easily through it, the *inferior part of the capsular ligament, however, (the part corresponding with the axilla), was perfect.*

The great blood vessels and nerves lay to the sternal side of the head of the humerus, and were forced a little out of their course. The axis of the head of the bone, in its dislocated position, was scarcely a quarter of an inch higher than the axis of the glenoid cavity.*

(To be Continued.)

EXPLANATION OF PLATE I.

FIG. 1.

- A. Deltoid.
- B. Scapula.
- C. Head of the Humerus.
- D. Shaft of the Humerus. The Biceps and Coraco-Brachialis before, and the Triceps behind the bone.
- E. Subscapularis.
- F. Teres Major.
- G. Latissimus Dorsi.
- H. Pectoralis Major.
- I. Coracoid Process.
- K. Edge of the torn Capsular Ligament.
- L. Some fibres of the Subscapularis embracing the neck of the bone.

FIG. 2.

- A. Shaft of the Humerus.
- B. Clavicle.
- C. Spine of the Scapula.

* The plates accompanying the concluding portion of Mr. Crampton's paper not being quite finished, we have deferred its publication until the succeeding number.—Ed.

- D. Supra-Spinatus.
- E. Infra-Spinatus.
- F. Greater tubercle of the Humerus torn off, and adhering to the united tendons of the supra and infra-spinatus and the teres minor Muscles.
- G. Triceps.
- H. Teres Major.
- K. Broken edge of the Tubercle.

FIG. 3.

- A. Internal face of the Scapulæ.
- B. Neck of the Scapula.
- C. Coracoid Process.
- D. Depression between the head of the Os Humeri and the greater Tubercle.
- E F. The line of direction of a force acting on the head of the dislocated bone, before it has been disengaged from the neck of the Scapula.

ART. VII.—*Contributions to Thoracic Pathology.* By WILLIAM STOKES, M. D., Physician to the Meath Hospital and County of Dublin Infirmary, Corresponding Member of the Medico-Chirurgical Society of Berlin, and of the Hunterian Society, Member of the Association of the College of Physicians of Ireland.

I. Notice of an hitherto undescribed termination of Pneumonic Inflammation.

II. Researches on the diagnosis of Empyema.

III. Researches on the diagnosis of Pericarditis.

Before I detail the results of some researches on the diagnosis of Empyema and Pericarditis, to which this paper is principally devoted, I shall give a brief notice of a case of pneumonic in-

flammation, as it is illustrative of a termination of the disease, which has not hitherto been described.

A young man entered the Meath Hospital on the 13th of April, 1832, labouring under the usual symptoms of acute pneumonia, which were of three days' standing ; the disease was found to occupy the lower lobe of the left lung, which had passed into the stage of hepatization. We employed general and local bleeding, and put him on the use of free doses of calomel and opium. The blood drawn did not present any inflammatory appearance, and although his general symptoms seemed alleviated, yet the stethoscope did not shew any improvement in the condition of the lung. In the course of the third day of his treatment, a violent exacerbation took place, subsequently to which a moist crepitus was heard mixed with the bronchial respiration over the dull portion, and the right lung became affected with a general bronchitis. These circumstances combined with the fact that no ptyalism whatever had occurred, induced me after two days to make the diagnosis of the third stage of pneumonia. About this time a remarkable change took place in the stethoscopic phenomena. A large rale was heard about the root of the lung, and the bronchial respiration here assumed so much of a cavernous character, that even after repeated examinations I declared to the class, that I could not satisfy myself of its exact nature, and therefore, could not say whether or not an abscess had formed ; the dullness continued. On the seventh day copious sweatings, preceded by rigors supervened ; these continued till the twelfth day from his admission, when he sunk.

On dissection, we found the bronchial mucous membrane universally inflamed, and recent adhesions of the pleuræ, particularly the left. On removing the left lung, its upper lobe was found crepitating, though engorged, but the lower, when viewed externally, represented a bag of matter, the yellow colour of which was seen plainly through the pulmonary pleura. This being opened, displayed the substance of the lower lobe com-

pletely dissected from its pleura, by the suppurative inflammation of the sub-serous cellular membrane. This process also was found to have invaded extensively the inter-lobular, and inter-vesicular cellular tissue, so as to cause this part of the lung to represent nearly the structure of a bunch of grapes. All these nearly isolated lobules were surrounded by puriform matter, in which they hung from their bronchial pedicles. There was no air in the cavity thus formed within the pleura, yet external to the lung, nor could I find any evidence of any bronchial communication with it.

The annexed plate, the drawing for which I owe to the kindness of Mr. John Hamilton of the Meath Hospital, a gentleman to whose high medical attainments I feel proud to bear testimony, will give a correct idea of this curious pathological alteration.

A. Represents the pleura pulmonalis slit open.

B. A portion of the diseased pulmonary tissue everted from its pleural investment.

C C C. Some of the nearly isolated lobules, shewing the pulmonary vesicles.

I have not been able to find any description of this termination of pneumonia, in the late works on pathological anatomy. The case is one which may be well appealed to by those pathologists, who hold that the original seat of pneumonic inflammation is in the inter-lobular and inter-vesicular cellular tissue, and that the air cells are secondarily affected. Here we find the sub-serous and inter-vesicular tissue extensively suppurated, so as to present a beautiful dissection of the lung, while the pulmonary vesicles were comparatively intact, but remained, as represented by the bunches of granules, immersed in the surrounding puriform matter.

Without drawing any general conclusion from the above case, it may be observed, that should future observations prove the truth of this doctrine, we shall have less difficulty in understanding how a lung, that has passed into the third stage of in-



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CASE OF PNEUMONIA BY DOCTOR STOKES

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flammation should yet recover from the disease, and resume its natural functions. With respect to the difficulties of the stethoscopic diagnosis, as to the question of the existence or non-existence of a pulmonary abscess, I shall only remark, that the observation of future cases must decide how far the occurrence of puriform matter in quantity, between the bronchial tubes and pleura, may modify the character of bronchial respiration.

DIAGNOSIS OF EMPYEMA.

Notwithstanding all that has been written on the diagnosis of empyema, as drawn from physical signs, yet every student and indeed many practitioners, must have felt the extreme difficulty that exists in certain cases, in determining on the existence of a sero-purulent fluid in quantity, within the cavity of the pleura. The importance of a correct decision of this question, whether we consider the safety of the patient, or the reputation of the physician or surgeon, of course adds to this difficulty, which I have often felt to a painful degree. If the following observations be found to remove the difficulties of this diagnosis in any case, I shall consider myself fully rewarded.

The diseases with which empyema, as to its physical signs may be confounded, appear to be the following.

Enlargement of the liver, particularly of the right lobe, by which the diaphragm is pushed upwards.

Hepatization of the lung.

Extensive effusion into the pericardium.

In most cases, then, it will be among these four affections, *empyema*, *hepatic enlargement*, *solidified lung*, and *effusion into the pericardium*, that the practitioner, in his diagnosis, will have to decide.

Now when we compare the two sides of the thorax, we shall find that there are much fewer difficulties in detecting an empyema in the left, than in the right side, for the following reasons. In the first place, when the effusion is at all extensive, we have the remarkable and nearly unequivocal sign of displace-

ment of the heart, which, under these circumstances, often pulsates at the right side of the sternum ; while the displacement from the disease on the right side, only occurring when the effusion is very great, and being in the opposite direction, is of course less striking ; and indeed may be often wholly overlooked. In the second place, the sign of dilatation of the side has much greater value, when observed at the left, than at the right side. In fact, in the cases where a want of symmetry exists between the sides, the right is almost always the larger : so that, when we find a dilatation of the left side, the value of the sign is of course increased, just as the value of broncophonia, as a sign of disease, is greater in the left than in the right lung, from the greater *natural* resonance at the right side.

Thirdly, when the disease occupies the left side, we escape all the embarrassment which the possible existence of hepatic disease causes in the diagnosis of an empyema of the right side. The volume of the spleen being so much less than that of the liver, its effect in modifying the phenomena of the thorax is very slight, and even when it is enlarged, the increase of volume takes place much more at the expense of the abdominal than the thoracic cavity, for reasons which are easily appreciated. It is right however to remark, that some cases have been observed in which an enlarged spleen produced a more or less extensive dullness in the lower portion of the left side, and Andral has shewn that such may occur without a distinct tumour in the left hypochondrium. A great enlargement of the left lobe of the liver has been found to have produced the same effect ; see a case in the *Journal Hebdomadaire*, vol. vi. 1830 ; such cases however as these are comparatively rare.

Empyema of the right side.—It has long appeared to me that the difficulties of this diagnosis have not been sufficiently dwelt on in medical works, and I can say, that even at the present day, the disease is commonly mistaken for chronic hepatitis. *I know of numerous cases, where, on dissection, the liver*

was found not enlarged, but merely displaced by an empyema, the existence of which had been wholly unsuspected during life. It is easy to understand how this mistake should be often committed; if we look at the symptoms there is a considerable similarity between those of a chronic pleuritis and a chronic hepatitis. The patient complains of a pain in the lower portion of the side, with a sense of weight; a dry cough; there may be an icteroid tint; there is a tumour in the right hypochondrium, and dilatation of the side. All these symptoms may be caused by either affection, and in a such a case too many practitioners, acting on the prejudice of the day, could see nothing but a profound hepatic disease. Again, if we consider the most evident physical signs, similar difficulties will be found to exist. Both diseases produce a tumour in the right hypochondrium, with dilatation of the side; both cause dullness of sound on percussion in the lower portion of the right side, and feebleness, or absence of respiration, without resonance of the voice, so that in many respects the symptoms and physical signs are so very similar, that the diagnosis is one often of extreme difficulty. I trust that the following observations may, in some cases, be found to throw light on the question.

We shall suppose that the patient presents the following group of symptoms and signs. The right side is dilated, and the patient feels a sense of weight in that situation. There is a tumour, evidently hepatic, felt in the right hypochondrium. The lower portion of the right side of the chest, both anteriorly and posteriorly, sounds dull, with absence of respiratory murmur, bronchial respiration, or resonance of the voice; all these, as we have seen, may arise from either affection. Now if the case be one of enlarged liver, we shall observe the following circumstances.

1. Although the side is dilated, yet the intercostal spaces and ribs present their usual relations, and *hence we have a marked dilatation, with the hollows of the intercostal spaces*

distinctly marked. This contrasts strongly with the smooth rounded appearance of a side dilated by an empyema.*

2. The tumour in the hypochondrium is either an enlarged or a displaced liver. If it be the first, we find the tumour presenting a continuous surface and feeling of resistance from its most prominent portion, to where it can no longer be traced under the ribs, the lower margin of which seems tilted out. But if it be a displaced liver, *we find, between its most convex portion and the edge of the false ribs, a sulcus, evident to the sight and to manual examination, presenting much less resistance, and evidently the result of the space left around the point of contact of two convex bodies, one the upper portion of the liver, the other the most prominent point of the depressed diaphragm.*

The grounds on which I put forward this observation are, first, the fact of my finding it admirably verified on a patient lately in the Meath Hospital, who laboured under an empyema of the right side, and in whom, on his recovery, the hepatic tumour, i. e. the displaced liver, *rapidly disappeared, with the return of clearness of sound, and the occurrence of extensive "frottement" over the lower portion of the left side;* and secondly, the result of injecting fluid into the cavity of the pleura in such quantity as to displace the liver, when it will be found that, as might be expected, we shall observe a similar appearance of the hepatic tumour. The best apparatus for this purpose is a Read's syringe and flexible tube; the opening in the chest (the best situation for which seems to be the supero-anterior portion) should be surrounded by wax, which has been poured into it in a fluid state. In this way great quantities of fluid can be injected into the pleura, so as to cause extreme pressure on all the surrounding parts; and if the belly has not been previously distended, and if the muscles are not rigid, the peculiar appearance of the

* See Report of the Meath Hospital, Dublin Hospital Reports, vol. v. This difference in appearance is evidently the result of the pressure being in one case caused by a solid, in the other by a fluid.

hepatic tumour can be studied easily. If we now open the abdomen the liver will be seen pushed downwards, and to the left side, from the pressure of the convex diaphragm, at the right of the broad ligament. Of course the direction of the inter-lobular fissure is altered; it now passes obliquely across the mesial line, and where this could be felt during life, it would, of course, be an additional diagnostic.

In these cases of mere displacement of the liver, the size of the tumour, of course, never exceeds the natural volume of the organ. This can be ascertained by measurement and the pleximeter. In a single case I was enabled to verify the diagnosis both of empyema and enlarged liver. The signs of empyema were unequivocal, particularly the dullness over the sternum, a point to which I shall return in the course of this paper; but the size of the abdominal tumour was so much beyond that of the ordinary volume of the liver, that there could be no doubt of the enlargement of this organ. The fluid in the thorax made its way externally, after the occurrence of an enormous anthrax in the postero-inferior portion of the chest, and the patient sunk after the discharge of vast quantities of fluid, and the occurrence of pneumo-thorax by external fistula. On dissection the liver was found nearly three times its natural size.

Another distinction, which I have found applicable in many cases, is drawn from the consideration, that when an extensive empyema exists, sufficient to dilate the chest and displace the liver, the right portion of the diaphragm no longer descends during a forced inspiration; for two reasons; one, the non-expansion of the affected lung, the other, the fact that it has already, not only attained, but passed the line which its contraction could attain; *it has become permanently convex in the opposite direction.* Hence on the patient's taking a deep breath, there is no descending motion of the lung. Now the reverse of all this occurs in cases of enlarged liver, *and we shall often find that in this case the postero-inferior portion of the right side, which was dull on percussion during ordinary breathing,*

becomes clear when a deep inspiration is performed. In the case of empyema, the contraction of the diaphragm on this side (supposing it did occur) would have the effect of increasing the extent of dullness over the posterior portion of the chest, in fact there would be increase of dullness upwards. But in the case of enlarged liver, the contraction of the diaphragm (which we know does occur) diminishes the extent of dullness, causing increase of clearness downwards.*

The last point to which I shall allude, as distinguishing empyema from enlarged liver, is connected with the mode of displacement of the heart, when such an occurrence is observed. In consequence of the pressure being exercised, in these cases, in different directions, this viscus assumes a different situation in both. When the displacement is caused by an empyema, the pressure being lateral, and the liver being depressed, the displacement, as has been long known, is towards the left side, without any remarkable ascent of the organ in the cavity of the thorax. But when an enlarged liver is the cause, the displacement is from below upwards; and we have seen cases where the heart was found to pulsate as high as the third rib.

Such are the observations which I have to offer on the diagnosis of these affections. It is plain, that two of the most important of them, namely, the condition of the intercostal spaces, and the appearance of the hepatic tumour, may be found inapplicable in certain cases; as where the patient is very fat, or anasarctous; or, lastly, where there is a complication with enlarged liver. These, however, are rare cases. The three following observations apply to the diagnosis of disease in either pleura, and, like the former, possess the value of being applicable even to those who are unacquainted with the use of the stethoscope.

When the hand is placed on the chest, while the individual who is the subject of observation is speaking, we feel a vibration

* It is scarcely necessary to remark, that this source of diagnosis is not available in cases, where, from the distention of the intestines, the descent of the liver is prevented. The indication in such a case is obvious.

on its surface, generally strong in proportion to the depth of the voice. When the stethoscope is applied, the natural broncophonia is audible. Now it has been long observed, that when the lung is solidified, the broncophonia is increased, and we have accordingly often been enabled to point out the most diseased portion of the lungs in cases of phthisis, by merely applying the hands over the scapulae while the patient was speaking. Now the same resonance of the voice, and corresponding vibration, are heard and felt in cases of hepatization of the lung from pneumonia, but do not occur at all in cases of empyema. The rule therefore is, if on placing the hand over the affected side, and making the patient speak with a tolerably loud voice, we feel a strong vibration in the part, the case is not one of empyema. It is plain that this will not assist us in the diagnosis of empyema from abdominal tumours, but it will do so from other diseases of the chest. How far the occurrence of an adhesion, sufficient to give bronchial respiration, may influence this result is still to be investigated. This very simple and valuable observation I owe to Mr. A. Hudson, my clinical clerk, from whose knowledge and acumen I have derived the greatest assistance in my researches.

The sound produced by the rubbing together of two serous surfaces, when in a state of disease, has been accurately described by Laennec, under the term "*Frottement*," or the sound of friction. This he states to be audible, in cases of effusions of air, beneath the pulmonary pleura, as in emphysema of the lung, and also in cases of dry pleurisy, observations which have since been fully verified. Collin also declared, that in certain cases of pericarditis, where the surfaces of the serous membrane were only separated by an effusion of lymph, a similar phenomenon was observable, a statement, to the truth of which I shall presently bear testimony. It occurred to me in two cases of pericarditis, not only to hear this sound of "*frottement*" most distinctly, but also to feel a manifest rubbing of two rough surfaces, one on the other; and having observed many cases where the sound of

"*frottement*" occurred in dry pleurisy, in several of which the diagnosis was verified by dissection, I determined to try whether, as in the cases of pericarditis, I could actually feel the rubbing of the two rough surfaces of the pleura, when in an analogous condition, and can now state with confidence that this can be done, and that the sign may be so plain as not to be mistaken by the most inexperienced person.

The first case in which this was observed, was that of a man who was admitted labouring under a scrofulous abscess at the lower portion of the sternum, and also presenting all the signs of an extensive effusion into the right pleura, evidently the result of a chronic inflammation. There were displacement of the liver and heart, dilatation of the side, elevation of the intercostal spaces, absence of respiration, and complete dullness. This patient recovered rapidly after the employment of a revulsive treatment. Clearness of sound returned, and at this period a distinct *frottement* became audible, from the spine of the scapula downwards. On placing my hand over the postero-inferior portion, I found, that when the patient took in a deep breath, the friction of the rough surfaces was most evident. Many of the class satisfied themselves of it, and indeed, the sensation was perceptible to the patient himself. In the course of a few days these phenomena gradually subsided, and the patient was discharged cured: since then we have been able to feel the friction in other cases.

This observation is of value, as indicative of the removal of the fluid parts of the effusion, and of course the progress towards cure. It strikes me also that by this means we shall be able to trace the organization of the false membrane. It is obvious that the subsidence or cessation of this feeling of friction can occur but in two cases; one the re-effusion of fluid so as to separate the pleuræ; the other the organization of the lymph. If then we observe its cessation, while at the same time the chest continues on percussion clear, we have an indication of the latter occurrence.

This principle we shall find is also applicable in the diagnosis of the progress of pericarditis. Although the value of percussion has been long recognized in the diagnosis of empyema, yet I do not find that the following observation has been hitherto noted. It is of importance as assisting in enabling us to distinguish between empyema and solidified lung.

When one lung is solidified either from pneumonia or tuberculous development, we find, that the portion of the thorax immediately corresponding to the seat and extent of the disease sounds dull. But as in solidification there is no increase of volume, we can understand how in the case of extensive solidity of one lung, that the half of the sternum corresponding to the affected side should be dull, while the other remained clear. In other words, that on one side of the mesial line of the sternum there would be clearness, while on the other, the sound would be dull. Now this is what really occurs, but we find the very reverse in cases of extensive empyema. Here the affection of one side produces not only dullness of the whole sternal region, but even beyond it, the obvious result of the displacement of the mediastinum. Dullness of the whole of the sternum, and even for some distance beyond it, is then a characteristic of copious effusion into the pleura.

The last observation which I shall make on this subject, relates to the sign of dilatation of the side. This has been described as always occurring in the early periods of the disease, and gradually increasing until the process of absorption commences. But I believe that we may meet cases of empyema *with a contracted side, from the very commencement of the disease*. I have seen one of these, and have received the description of another drawn up by Mr. William Hamilton, which occurred under the care of Mr. Cusack.

In both these cases, the disease arose from an injury of the chest. In the first, the patient received a musket-ball in the left inter-scapular region, which perforated the lung, and lodged anteriorly, close to the insertion of the diaphragm. This

patient suffered exquisite and increasing pain, from the time of his wound to the period of his death, which occurred in a little more than a fortnight after the accident. The slightest attempt at extension of the side, brought on such suffering, that he constantly kept himself bent down to the left side; and when I saw him, the side was nearly two inches less than the opposite one. This circumstance, together with the occurrence of a bronchial respiration in the postero-inferior portion, caused great doubt in the minds of some of his attendants, as to the existence of an empyema. I determined, however, that the case was of this description, from the circumstance of the heart being displaced, and pulsating strongly at the right side of the sternum. The operation was performed, but was unsuccessful, in consequence of the operator being misled by the great contraction of the side, and making his incision too low. In fact, the knife entered below the diaphragm. The patient sunk on the following day; and on dissection, a vast collection of matter was found in the left pleura, which was extensively covered with coagulable lymph. The ball had divided the lung from above, downwards and forwards, and its trajet was greatly dilated by the effusion, and lined also by lymph.

In the case drawn up by Mr. William Hamilton, the patient had suffered a fracture of the fourth, fifth, and sixth ribs; had exquisite pain of the side, and total inability to extend it. This man after twenty-four hours of suffering, presented all the stethoscopic indications of copious effusion. He was ultimately discharged, but throughout the whole of his disease, the side was contracted. The facts of this case, as recorded here, may be fully relied on. Mr. Hamilton's attainments in stethoscopic investigation are a sufficient guarantee for their accuracy.

If the foregoing observations on the diagnosis of empyema be found to assist the practitioner in its detection in any case, I shall feel more than rewarded, and it is scarcely necessary to observe, that their value is increased from the diagnosis, found-

ed on them, being attainable even by those who are not versed in auscultation.

DIAGNOSIS OF PERICARDITIS.

The diagnosis of this disease, being founded, as Louis has well remarked, chiefly on negative evidence, any addition to our knowledge of its direct signs must have value. On this subject, however, I have nothing original to offer, but may state that in several cases I have been enabled to verify the observation of Collin, relative to the sound of friction, produced by the rubbing together of the surfaces of the pericardium, when they are covered with unorganized lymph, a sound which he compares to that of the creaking of new leather. But I can state further, that numerous modifications of this sound may be met with. It resembles the creaking of leather in some cases, the rubbing of two very rough surfaces in others. In some it may be confounded by an inexperienced observer with Laennec's "Bruit de Rape," and in others, may closely resemble the different modifications of the bruit de soufflet, with which I feel satisfied it has constantly been confounded. In many instances we have observed that the sounds in a single case have undergone a series of modifications, passing from the loudest sound of friction of two very rough surfaces, to a sound similar to the most faint bruit de soufflet; and in two cases we have (on applying the hand) felt the friction of the roughened surfaces of the pericardium in the most distinct manner, causing a violent "fremissement." In one of these cases we verified the diagnosis by dissection, and found the surfaces of the pericardium completely covered with a very dense coating of lymph, raised into rounded projections of several lines in diameter, and incorporated with the serous membrane. This was evidently the result of a chronic inflammation, and in one situation a strong adhesion was found. The cause of death seemed to be a recent attack, as the urgent symptoms were not of long standing, and

a quantity of bloody lymph existed over the posterior and lateral portions of the heart.

It has occurred to me to see patients who had before enjoyed a good state of health, after an attack presenting all the characters of an acute, but not very violent pericarditis. They had still some pain about the heart, palpitation, increased action of the organ, and irregular pulse; and on applying the stethoscope over the heart, I could hear a sound which many would call *bruit de soufflet*, but which I apprehend is produced not by any valvular disease, but by the friction of the two surfaces covered by lymph, as yet unorganized. I have been able to remove this sound by free leeching and counter-irritation in these cases; and believe that there is the most complete analogy between it and the *frottement* of Laennec, as arising from inflammation of the pleura.

Some time ago I saw a patient of Dr. Thwaites's, who presented the phenomena of hypertrophy of the right ventricle, with a sound so circumstanced, that I conceive there might be a narrowing of the right auriculo-ventricular orifice. On dissection, however, it was found that no such narrowing existed, but that the cavity of the pericardium was obliterated by adhesive inflammation. This interesting case has been published in the fifth number of this Journal. Now I feel almost certain, that in this case I mistook this sound of friction for a *bruit de soufflet*, depending on a valvular disease; and this opinion is borne out by a fact stated by Dr. Thwaites, that on his first examination the *bruit de soufflet* was heard over the whole precordial region, while on mine, which was more than a year subsequent, it appeared more partial. This the progress of the obliteration would account for; and, it is stated, that on dissection a portion of the surface, at the postero-superior portion of the left ventricle was found, where the morbid action seemed more recent.

The healthy condition of the valves, in a case where loud and varying *bruit de soufflet*, with *fremissement*, existed, is

strongly illustrative of my views on this subject. I am at present occupied in researches on this subject, the object of which is to distinguish this sound from the bruit de soufflet, produced by valvular disease. As yet I have arrived at but two results; one its rapid removal (in certain cases) by an antiphlogistic treatment, which allows of the organization of the lymph, the other, that after it has been removed by treatment, a temporary excitement of the heart will not reproduce it, as we know occurs in cases of valvular disease. Its intensity too, varying with the extent of dullness, is another sign, as connected with the quantity of liquid effusion into the cavity of the pericardium.

I have lately had the pleasure of meeting with a most intelligent American physician, to whom I showed some of my drawings and preparations illustrative of these views. He completely coincided with me, and mentioned several cases which he had lately witnessed in Paris, in which the diagnosis of Collin had been verified.

ART. VIII.—*Observations on Mediate Auscultation, as a Practical Guide in Difficult Labours.* By WILLIAM O'BRIEN ADAMS, M. D., Fellow of the King and Queen's College of Physicians in Ireland, and one of the Assistant Physicians to the Dublin Lying-in Hospital.

THE introduction of the stethoscope, as a means of arriving at certainty of diagnosis in diseases whose phenomena from their very nature could not but be obscure, has deservedly been hailed as one of the greatest benefits conferred upon medicine in modern times; it has, in fact, in a great measure redeemed the practice of medicine from the opprobrium of a conjectural art, bringing, as it were, under the cognizance of our senses, the secret operations of nature, under the influence of disease. By it we are as capable of recognizing the changes which inflamma-

tion is producing in a lung, with as great certainty, as a surgeon is of ascertaining the presence of a stone in the bladder, by the use of the catheter or sound; by it we can not only ascertain the existence of effusion into the cavity of the chest, but its quantity and extent; in fact, a certainty of diagnosis, in diseases of the chest, has resulted from the application of this instrument, as unequivocal to an experienced stethoscopist, as if the morbid alterations of structure were actually submitted to ocular demonstration. But the field of usefulness of this instrument has been considerably extended, by its application to practical midwifery, and were we to measure its advantages by the information which it supplies, and which could not be furnished from other sources, we should not hesitate to say, that the amount of benefit, from its application to this department of medicine, at least equals, if it does not exceed, all other uses to which it has been heretofore applied. In this department alone do we find ourselves so situated, that we are called upon at the moment to decide upon a practice, the consequence of which directly involves the life of a human being: in other cases symptoms and stethoscopic phenomena lend their aid to each other, but in the case to which I at present allude, we are as it were, altogether dependent upon the information which the stethoscope supplies, to prevent us needlessly exposing the mother to the risk of a protracted labour, under the too often false idea, that the child is still alive; for by observations drawn from the careful, and repeated employment of this instrument, we are assured of the fact, that the child has ceased to live, at a period long before the time to which operation was formerly deferred. Any thing which claims a pretension to being a guide in the direction of difficult labours, from the importance which the subject involves, is entitled to no moderate share of attention from the accoucheur. Authors have laid it down, that any labour not completed within twenty-four hours after the os uteri has begun to dilate freely, the head of the child presenting, may be termed a difficult labour. This definition is liable to objection, as greater difficulties may be surmounted by one woman in a

few hours, than by another in many; in a general way it may be stated, that labours are rendered difficult, either by the degree of obstruction, or by the insufficiency, or absence of the power, by which the obstruction should be overcome, or in other words, some are referrible to the mother, others to the child. Other causes of difficult labour have been assigned viz., an imperfect discharge of the waters, shortness of the funis, fever, weakness of the constitution, passions of the mind, suppression of urine, stone in the bladder, enlargement of the ovaria, &c. But beside the circumstances already mentioned, there is one of much more frequent occurrence, which by all means in our power should be carefully guarded against, we allude to the ill-timed and improper management of a labour, and the consequent derangement of the order of its phenomena; in conducting a difficult labour, much foresight and decision is required to point out, when the assistance of art may be necessary, or when we may safely depend upon the efforts of unassisted nature. And in this dilemma the stethoscope affords us the most unerring guide, for by it we are enabled to pronounce with certainty on the life or death of the fœtus.

Writers on midwifery enumerate the following symptoms, as indicating the death of the fœtus in utero; want of foetal motion, and pulsation at the fontanelles, a rolling as of a lump or dead weight to that side on which the mother is lying, shivering fits on the part of the mother, flaccidity of the breasts, fetor of the uterine discharges, an emphysematous state of the scalp, and a very loose feel of the bones of the cranium. These symptoms may assist our diagnosis before labour has commenced, but certainly are of no value once labour has set in, for every one of them may be absent, and yet the child be already dead. But we would submit that the only certain criterion of deciding on the life or death of the fœtus, is the absence or presence of the peculiar double beat of the foetal heart, which is easily discovered by a person accustomed to the use of the stethoscope. In this situation, perhaps the most interesting, and often the most embarrassing, in which the medical practitioner ever finds himself,

the stethoscope, by affording him evidence the most unequivocal of the still persisting life of the foetus, guides him in the adoption and execution of means best calculated to lead the case under his care to a favourable termination. If then we have once heard the foetal heart, and after a time, having again instituted a most accurate examination, (nor should the accoucheur, under circumstances calculated to arouse his suspicions, that the labour might be protracted, ever omit making this necessary examination), find that this peculiar sound is no longer audible, we have in my mind positive evidence of the death of the foetus, and that the time has arrived, which not only justifies, but demands the scientific use of instruments. This in fact is, and has been the desideratum so long sought for by practical accoucheurs, constituting one of the most difficult, and important parts of our professional duty ; lest on the one hand we should have recourse to instruments precipitately, and unnecessarily sacrifice the life of the child ; or on the other, defer their application too long, till serious injury to the mother has actually occurred, or perhaps even her death become inevitable.

Symptoms, however, may arise in the progress of a difficult labour, requiring delivery before the foetal heart has ceased, such as spitting of blood, convulsions, uterine hæmorrhage, emphysema, the existence of aneurism, and great tenderness of the abdomen on pressure, &c., &c., but these causes for instrumental delivery, may in some respects be considered accidental. At present we mean to confine our observations to the causes which are immediately connected with the powers of expulsion, as unaltered position of the child's head, either from impaction, or from, as termed by the French writers, "*la tête arrêtée au passage*," or from cessation of labour pains from exhaustion.

Of twenty-three cases of difficult labour, requiring the child's head to be lessened, which came under my observation during the past year, the periods of labour varying from twenty-six to fifty-six hours, many of which had been several days in labour before they came under medical treatment, in

nineteen of them, the foetal heart having been distinctly heard with the stethoscope, had ceased, it was therefore neither necessary nor expedient to delay delivery ; of the other four cases three required delivery from sudden exhaustion and debility setting in, and the fourth had been seventy-two hours in labour of her tenth child ; the following are the particulars of her case.

Elizabeth Delaney, aged 36, was admitted into the Lying-in Hospital on the the 20th of January, 1832, reported to have been two days in labour before admission, at which time the head of the child was high in the pelvis, and pressing strongly on the pubis, where it continued for twenty-four hours, during which time the patient suffered the greatest distress, the uterine action being almost constant. She had been twice forced delivered in this hospital before. Dreading rupture of the uterus, the head was lessened, and the delivery effected with the crotchet, as the head still remained so high, as to be little more than within reach of the finger. In one of the other three cases, was that of Eliza Sweeney, aged 30, admitted into this hospital, on the 3d of August, 1832, being her second pregnancy. This patient was thirty-six hours in labour, during which time, the pains were feeble, yet the head of the child made considerable progress, her pulse remaining quiet, and no symptoms indicating present danger ; the child's heart was also quite audible with the stethoscope ; suddenly, however, the most alarming debility set in, and the pulse could scarcely be felt at the wrist ; there was some vomiting, accompanied with much pain over the uterus on pressure ; these symptoms induced a strong suspicion of the uterus or vagina being rent, the immediate delivery was therefore deemed necessary, the head was lessened, and the child brought away with the crotchet. She died on the 5th of August, and on post mortem examination, an opening was found at the junction of the uterus with the vagina at its posterior part, exactly opposite to the promontory of the sacrum, which was not larger than to permit the passage of one finger through it ; the muscular substance of the uterus at its anterior part, where it joins the va-

gina, had also given way to a considerable extent, but the peritoneal covering remained uninjured ; the pelvis measured scarcely three and a half inches, in its antero-posterior diameter ; this woman had been forced delivered in this hospital eleven months ago, of her first child. Another of the three was that of Alley Byrne, æt. 24, who was admitted on the 24th of November, 1832, being her first pregnancy. This woman complained much of pains in her back and abdomen, from the time of admission till the 27th, without there being any dilatation of the os uteri, at which time the os uteri began to dilate, and on the 28th, 9 o'clock, A. M., the os uteri was not fully dilated, but in consequence of her pulse being nearly imperceptible, and the woman being much exhausted, it was thought necessary to lessen the head, and accomplish the delivery with the crotchet. This case had been most carefully examined by Dr. Collins and myself twice on the 27th, but we were unable to detect the foetal heart. The third case was that of Rose Penney, æt. 26, who was admitted into this hospital on the 16th of March, 1832, in her second pregnancy; and reported to have been three days in labour before admission ; eighteen hours after which it became necessary to lessen the head. From the time of admission until about twelve hours before she was delivered, the uterine action was feeble, and the labour made no progress whatever ; the os uteri was not more dilated than the size of half a crown ; when the uterus began to act strongly it gradually yielded, and as the head came nearly to press upon the perineum, its progress was prevented by a firm and unyielding band in the vagina, which appeared the chief obstacle to the passage of the head ; the edge of the band was slightly cut with a blunt pointed bistoury ; some hours after this was done, the head was found to have advanced, yet the band opposed it strongly, but as the foetal heart's action had now ceased, which was becoming indistinct for some hours, the head was lessened and completely broken down, (being allowed to remain in that state for three quarters of an hour, to permit the parts to dilate slowly by its pressure), and was then brought away without difficulty. We should remark, that this

woman had a slight attack of convulsions during the progress of labour ; at the time of delivery her pulse was 120, and she appeared very feeble and exhausted.

Thirteen cases of the twenty-three were first pregnancies, and in four, the funis prolapsed in the progress of the labour.

We feel we are warranted in inferring from this statement, that the foetus in utero can only sustain a certain degree of pressure, and that only for a limited time ; but from what we have said in a former part of this paper, the number of hours a woman has been in labour, cannot be taken as a guide ; the phenomena which take place when the *secale cornutum* is exhibited in tedious labour, go to confirm this point, as it has been stated, that in almost every case in which the ergot is given, the child is still-born ; it also appears, that the foetal heart ceases, in most cases, before the assistance of art is required, and before any very urgent symptoms arise on the part of the mother : by the aid of the stethoscope, we are enabled to determine, with accuracy, whether the time has arrived, which justifies the use of instruments. The events of difficult labours, either as respects the mother or child, depend, in a great measure, on the manner in which women are conducted through them ; to every practical man, cases will occur and a time arrive, when we are compelled of necessity to have recourse to the aid of instruments, for the more safe and speedy delivery of our patient ; and in the majority of such cases, we are but freeing the mother from imminent danger, by the abstraction of the lifeless body of her child. Most women are impressed with the idea, that in difficult labours, their safety in a great measure depends upon the skill of her attendant ; it is therefore necessary, we should act with the greatest caution ; for how often do we see in the progress of difficult labours, the great principle of their patience and resolution (namely, the affection of the parent for the child) quite overcome by their present suffering, and persuading themselves that the child is dead, beg to be freed from their pain and danger ; but the decision of the practitioner, to adopt such active

measures, involving the life of a human creature, must be supported by sounder reasons, and a better principle, than conformity to the inclinations of others, and here it is that the stethoscope affords a certainty of information whereby he can regulate his conduct under such embarrassing circumstances.

Pursuing this subject still further, mediate auscultation will direct us as to the instruments necessary to be used in each particular case ; for the forceps, an instrument which we see, even in the most experienced hands, cannot be employed without some risk, both to the mother and the child, are certainly, we conceive, inapplicable, when the foetal heart has ceased to be heard. In a case in which I used them lately, in every respect favourable for their application, and immediately before using them heard the foetal heart most distinctly, the operation was performed with the greatest ease, and did not take up more than five minutes, still the child shewed but little signs of life ; and certainly respiration would not have been established, but for the unwearied efforts of some of the attendants, who exerted themselves to the very utmost in the energetic employment of every means calculated to reproduce animation, which were successful at the end of two hours ; however, it survived but twenty-eight hours after birth.

The cases above enumerated may appear to some not sufficiently convincing of the necessity, or perhaps even utility, of the stethoscope in the practice of midwifery ; but its almost daily employment in the Dublin Lying-in Hospital, especially in the hands of Dr. Collins, the present efficient and able Master of this noble establishment, has afforded to all, who have witnessed the so frequently occurring instances of its application and value, ample evidence of its pre-eminent importance as a practical guide ; in fact, entitling it to be ranked amongst the very greatest improvements made in practical midwifery during the past century.

We would not, however, be understood to say, that in every case in which the foetal heart is inaudible, we would positively in-

fer the death of the foetus, as a case might by possibility occur, in which the heart was inaudible from position, tumours of the soft parts of the mother, &c., but such cases are of so very rare occurrence, as by no means to deserve to be looked upon as a practical objection to what we now propose. If the sounds of the foetal heart have once been observed, and during the progress of labour cease to be heard, then there can be little doubt of the death of the foetus.

We would wish to caution the young practitioner against some mistakes into which he might by possibility fall ; namely, the confounding the pulsations of the abdominal aorta of the mother with the foetal heart ; and also, that in some cases he will find the sound of the mother's heart transmitted along the abdominal parietes, so as to be audible by the stethoscope in the epigastrium, and the lateral parts of the abdomen and loins ; but a comparison of the sounds of the foetal heart, as heard through the cylinder, with the mother's pulse at the wrist, showing a vast difference in frequency between them, renders the mistaking the one for the other, in ordinary cases, quite impossible ; the foetal heart, which, in natural presentations, is generally to be heard in the right iliac or inguinal regions, perhaps a little towards the hypogastric, but in some cases in the opposite side of the abdomen, beats from 120 to 160 in a minute ; however, in the course of a difficult labour, the mother's pulse may rise to this, or nearly to this standard, therefore it requires the frequent use of the stethoscope, and some discernment, to distinguish between them, when the pulse at the wrist corresponds in frequency with what we suspect to be the sounds of the foetal heart ; the young practitioner, by frequent examination, should well accustom his ear to the peculiar double beat of the foetal heart, to enable him to pronounce with certainty in such difficult cases.

ART. IX.—*On the Contagion of Cholera.* By WILLIAM HARTY, M. D., Physician to the Prisons of Dublin, &c. &c.

HAVING from time to time been informed of sundry extraordinary proceedings in various parts of Ireland originating in fear and terror of the presumed contagious power of cholera, and being of opinion, that such fear was prematurely and inconsiderately instilled into the subjects of these realms by the constituted authorities, medical and political, I consider it my duty publicly to record a few observations, the result of personal experience, in a matter which involves not merely the cause of truth, but also, and in no small degree, the general welfare of the community.

The great question at issue, “whether cholera be contagious or not,” may be divided into two others, each more or less independent of the other: the *first*, whether the disease can be introduced into any place or country without the agency of contagion, or in other words, is contagion indispensable for its origin in any given place: and (should the answer be, that contagion is not necessarily required for its introduction) the *second* is, whether the disease, so formed and introduced, be capable, under any and what circumstances, of assuming a contagious property. On one branch of the general question I have formed a decided opinion; respecting the other I entertain serious doubts, and shall accordingly detail such circumstances, pro and con, as may enable others, under an accumulating series of authentic facts, to establish hereafter some settled conviction of the truth.

Cholera, which had commenced its English career at Sunderland in October, 1831, and had gradually advanced by a north-westerly course to Edinburgh and Glasgow, reached Ireland in the beginning of March, 1832, when a few cases of the disease occurred in Belfast, between which place and Glasgow a

considerable and constant commercial intercourse existed. Under these circumstances, the disease being elsewhere unknown in Ireland, its existence in Dublin was announced, about the 22nd of March, by several most respectable physicians, some of whom had witnessed the disease at Sunderland; and its occurrence in the Richmond Bridewell on the 25th, was on 26th personally communicated by me to several members of the then existing Board of Health. The board, however, took no official or public cognizance of the matter till the 29th, when they published their first bulletin, without having instituted any inquiry into the cases alleged to have occurred in the Bridewell. To the present hour I have not been officially questioned on the subject, nor was any official visit paid by order of the Board to Bridewell till the 31st March, and then under peculiar circumstances, detailed in the subjoined letter, which a sense of public duty, as well as of private wrong, induced me to address to that Board on the 3rd April. This letter exhibits the first bulletin of the Board, with appropriate comments on its very equivocal wording, and contains besides a statement of important preliminary matter, connected with the discussion of the agency of contagion in the introduction or propagation of this disease. In publishing this letter here, let me add, that subsequent experience has not led me to retract or qualify any of the statements or opinions it contains:

TO THE CENTRAL BOARD OF HEALTH FOR IRELAND.

11, *Middle Gardiner-street*,
April 3rd, 1832.

GENTLEMEN,

On the 29th of March you gave to the public your first official document respecting that grossly misnamed disease, the epidemic cholera: that report, together with those subsequently made, now lies before me; they evince an obvious and discreet reluctance on your part, to proclaim the existence of a disease, which, ac-

cording to your *published* opinions of its nature, must not only excite great alarm in the public mind, but place the intercourse with the port of Dublin under a system of ruinous restrictions. Though there are few circumstances which, in my judgment, can warrant a suppression of the truth, yet, in your very responsible position, and with your notions, if I cannot praise, I will not censure it; and so strongly did I feel disposed, at least not to counteract your views, that, though neither my experience nor reading permitted me to subscribe either to the facts or reasoning of your reports, it had been my intention to have addressed to you a *private* communication, to which it should have rested with you *alone* to have given any publicity.

Now, however, after the transactions of Saturday, (to which I shall hereafter refer), our relative position has been so far changed, that, valuing, *as I do*, the courtesies of civilized life, I cannot approach you except through the medium of the public; and whatever may be my reluctance *thus* to address you, I do so with the less hesitation, because the public interests are on this occasion far more deeply involved, and far more important, than either the character of the Board or that of the individual who presumes to call its decisions in question.

In your report of the 29th ult., you state, that, after examining evidence, both written and oral, "it appeared that the symptoms of the cases (reported to you) very much resembled those described as attending the epidemic cholera in England, BUT the Board were not able to trace the disease to any communication by which it might have been introduced into the neighbourhood of Dublin." Permit me, gentlemen, with all courtesy, to ask whether the legitimate inference you draw, and wish others to draw, from these premises be, that though the fatal cases reported to you had symptoms "very much resembling" the epidemic cholera, they could *not* be cases of that disease, *because* "the Board could not trace the disease to any communication by which it might have been introduced into Dublin?" If such, gentlemen, be your meaning, it is obvious that, without any personal experience of your own, you have already prejudged the great question at issue, and have decided not only that the disease is contagious, but that it can be propagated by contagion *alone*, and that you are therefore justi-

fied in doubting *at least*, if not in denying, that any disease now existing amongst us can be the epidemic cholera, *because* the agency of imported contagion cannot be traced in its production. If this, your prejudgment, be correct, your inference may, no doubt, be defended; *but* here allow me to ask, is this prejudgment (upon the correctness of which *so much* depends) justified by facts, or have facts been either sought after or heeded, which might in any degree tend to impugn such a decision? The fatal cases that occurred near Summer-hill, you have yourselves admitted “very much to resemble” epidemic cholera. The medical gentlemen who attended them never witnessed such a disease previously, and those who had seen cholera at Sunderland, after visiting these cases, entertained no doubt of their identity. Are we justified, either by our actual knowledge or by accredited facts, in doubting such identity, *because* the importation of contagion into Dublin cannot be traced? That these were genuine cases of the disease, I for one cannot hesitate to believe, notwithstanding the want of such proof, the absence of which in this instance I can the more readily credit, from my personal knowledge of facts I shall now proceed to detail, and which I feel it my bounden duty to submit, for the information of the public, and for the serious and timely consideration of the Government, to whose care the interests of that public are intrusted.

As physician to the prisons of Dublin, which office I have now filled for more than 20 years, I was called upon by the last Michaelmas Term Grand Jury to lay before them a statement of my views of the disease then prevalent at Sunderland, and of the measures I would propose for securing the health of the prisoners intrusted to my charge, in case Dublin should be afflicted with a similar visitation. I accordingly submitted to them a document such as they required; and, believing, as you do, the disease to be contagious, I proposed, amongst other things, to make such arrangements as would effectually protect the *Richmond Bridewell* against its introduction, though I despaired of repelling it from any of the other prisons. Under such circumstances, my astonishment may be easily estimated at finding in the hospital of that prison, on Monday morning, the 26th March, two females suffering under the formidable paroxysms

of a disease such as I had never witnessed till then, and to the immediate treatment of which I should have felt myself at the moment utterly unequal, but for the knowledge acquired by a perusal of the most esteemed works on the epidemic cholera. A third case precisely similar in its general characters occurred that evening, and since that five others, all females. The first patient was taken ill on Sunday evening, and was visited in my absence that night by my friend, Dr. Montgomery, to whose judicious and energetic treatment she is indebted for her safety. On the night of Monday I made my first official report to the magistrates of the Head Office of Police, by whom it was forwarded to Sir William Gossett, and by him laid, with my subsequent daily reports, before the Board of Health. In these reports I did not hesitate to express my belief that the disease was the same as the cholera, then epidemic in England; and yet, though these reports, expressing such opinions, were not only submitted to the Board, but that opinion personally made known on Monday to some of its members, I have not yet been officially questioned on the subject, nor was any official visit by their order paid at the Bridewell, to inspect the cases, until late on *Saturday*, at a time when I was absent and unapprised of such visit, when all the patients, save one, were nearly convalescent, or were placed under circumstances, such as would disable Hippocrates himself from forming a sound, or, indeed, any judgment of the previous illness.

Whence, it may be asked, this tardiness of inspection? whence this reluctance on the part of the Board to examine, inquire, and report? The question is, perhaps, not very difficult of solution: the Board, believing cholera to be contagious, and propagated solely by contagion, and therefore persuaded, that, should the existence of the disease be ascertained, the strict enforcement of quarantine could not be dispensed with, were naturally and very properly slow in declaring such a disease to exist, which must be followed by consequences so disastrous to trade, and so odious therefore to the whole community. Hence there should be tardiness of belief, yet no tardiness of inspection. "*Facile credimus quod volumus*," is a notorious truism: the reverse of the maxim is perhaps equally correct, and may be fully illustrated in the instance of these Bridewell cases. If these were not

cases of genuine cholera, as they were alleged to be, they ought to have been forthwith inspected, that the public mind might at once be disabused ; but if they were, what conclusions almost necessarily followed from their occurrence in the Richmond Bridewell ? why, neither more nor less than an highly important inference contravening the opinion of the Board, viz., that this disease must have originated there *independently of contagion*, and that quarantine, therefore, must be, as it has proved to be elsewhere against cholera, a useless and vexatious, and unnecessary restriction on commerce. What are the proofs ? Let facts, known to every officer of the prison, speak for themselves. The Bridewell contains about 200 male and 100 female prisoners, all sentenced to terms of imprisonment longer than six months, and divided into several distinct classes, without means of communication : its situation on the South Circular Road, near Harold's-cross, is well known ; none can be more healthful, nor can any public building be named that can exceed it in cleanliness and ventilation : the prisoners have no intercourse with the public, except once every second month, when such of their friends as call see them in the open air, through two iron gratings, in the presence of one of the turnkeys, and without the power of even shaking hands ; and on these occasions, such articles as oranges, cakes, &c., alone are permitted to be given, and that through the medium of the turnkey. The last visiting day was on the 5th of March : the *first* prisoner attacked had been an inmate of the prison for six months, and had *never* been at the visiting grate : the *second* had been there on the 5th, but had no intercourse whatever with the first, both being in different classes : the same was the case with the third, and of the majority of the remainder : they were all young, cleanly in their persons, and sober of necessity, and, with one exception, in perfect health, well fed and well housed ; not a single case of acute sickness in the prison. Will the most hardy, *exclusive contagionist* maintain that, under such circumstances, the contagion of cholera must, notwithstanding, have been introduced into the prison, though at the time the disease had not drawn a closer parallel to Dublin than Belfast, and though the few cases alleged to have occurred in Summer-hill were nearly simultaneous, and were on the north side of the city ? Or will he, in preference, maintain that, be-

cause the sickness could not be traced to any foreign importation, it could not be that disease? an *argument* which he will no doubt strengthen by the assertion, that it cannot have been cholera, as none of the sick have as yet been pleased to die!!

Having already run this letter to an unreasonable length, I am unwilling now to enter on further details, but shall conclude for the present by reiterating the assertion of my opinion, that the disease under which these females suffered, was one previously unknown to me, and to the many physicians and surgeons who saw it in its progress; that it was the same with the epidemic cholera now prevalent in England, cut short, indeed, in its death-dealing career by the most prompt and decisive measures; that it was *not* introduced there by contagion, and that it has *not* been propagated, *as yet*, by that agency, though numerous attendants have now, for ten days, been in the closest and most fearless contact with the sick. These, gentlemen, are assertions not lightly or hastily advanced; *assertions* which, if well founded, at once demonstrate the inutility of quarantine as a preventive against the introduction of cholera, and which, therefore, for the public interests, and for the relief of commerce, demand the strictest investigation. By the result of such an investigation, honestly and honorably conducted, I am willing to stand or fall in the estimation of my professional brethren.

I have the honor to be, Gentlemen,

Your obedient Servant,

WILLIAM HARTY.

Physician to the Prisons of Dublin, &c. &c.

We have in this letter an important fact established, (indeed it was scarcely disputed) that the Bridewell cases to the south of Dublin did not, could not owe their origin to contagion, and we have also the deliberate admission of the Board, *an admission* never subsequently recalled or qualified, that they were not able to trace the disease (simultaneously occurring at the north side) to any communication by which it might have been introduced into the neighbourhood of Dublin. Summer-

hill at the north, and Richmond bridewell at the south side of the Liffey, are at least two Irish miles distant from each other, with this sole resemblance in position, that they were both very remote from the port of Dublin, and were each near to, and nearly equi-distant from the Grand and Royal Canals which surround the city. Without referring to analogous facts alleged to have occurred at Paris and elsewhere, am I not justified in concluding, that as regards Dublin, this disease originated there independently of a contagious agency. If Cholera then did not owe its introduction amongst us to the agency of contagion, do facts subsequently occurring warrant us in affirming that the disease, however introduced or originating, assumed a contagious character, and was propagated thereby? In answer to this question, I shall first detail such circumstances as within my own knowledge may contribute to elucidate this subject, and shall then advert to matters within the experience of others.

Five prisons of Dublin, containing in all about 1000 inmates are under my professional charge: of these two only could be protected from a contagious disease prevailing in the metropolis: these were the Richmond Bridewell and Smithfield Penitentiary, to both of which, prisoners sentenced at sessions are transmitted from Newgate: this prison, as well as the Sheriff's Prison and City Marshalsea, (all in the same small street), are incapable of defence against such a foe, because, independently of the free access which their friends must ever be permitted to have to the untried prisoners in Newgate, there are upon an average twelve new prisoners daily consigned to its walls, a class by whose means fever never failed of being introduced into Newgate, whenever it was epidemic in the city. The case of the two debtors' prisons is precisely similar; new prisoners being daily committed, and the public having free access to them at all hours of the day. Now let us see what has been the history of Cholera in these several prisons. In the Bridewell it commenced on the 25th of March, and ended (after assailing ten females) on the 10th April: it made its appearance in Newgate

on the 18th April, and, between that date and the 23d July, attacked three men and two women, (one of whom died). It next assailed the Smithfield Penitentiary on the 29th April, and after attacking within a few days a boy and three women, two of whom died after removal to the cholera hospital, it was no more heard of there. In the Sheriff's Prison it did not make its appearance till the 6th May, when one man died of it in 36 hours, and the only other case in the same Prison, (remarkable for the intemperate habits of its inmates), occurred on the 26th November, which case also terminated fatally. Finally, it shewed itself in the City Marshalsea, (the filthiest and most crowded prison in Dublin, and containing the poorest and most intemperate class of inmates, in constant communication with the most diseased portion of the community), on the 7th July, when the sister of the keeper, whose actual residence was adjacent to, and not in the prison, was assailed, and died in 36 hours after; on the 10th, 15th, and 20th, of the same month, two of the male prisoners and one female were taken ill of the same disease, and were removed to the cholera hospital. Another was attacked in November, and with him the disease ceased its further ravages. In estimating the value of these facts, let it be understood, that during the whole period from April to September, cholera was raging in the whole neighbourhood around these several prisons, and that no system of preventive measures, (except general cleanliness, white-washing, and chloride of lime), was, or could, in these prisons, be enforced with sufficient strictness to guard against the introduction of a contagious disease. Indeed, I must confess, that after my early experience of cholera in Bridewell, I lost all fear of any contagious property belonging to it, though before I had an opportunity of witnessing the disease, my predisposition was to consider it contagious. I accordingly felt little anxiety to enforce any very strict measures for the exclusion of imaginary contagion from the other prisons, and, as the result proved, without any cause for regret. The experience of many years

had taught me, that whenever continued fever was epidemic in Dublin, it uniformly found its way into the prisons, by means of those newly committed, and more especially, and most readily, into Newgate and the City Marshalsea; and on some of these occasions prevailed most extensively in Newgate, simultaneously with the diffusion of the disease in the city. Now if we are to credit the assertion of facts alleged in proof of the contagion of cholera, it must, if contagious in those instances, possess a greater intensity and rapidity of character in that respect than fever, a conclusion quite at variance with all my experience of both diseases. How under such circumstances can we, consistently with the doctrine either of absolute or contingent contagion, account for the simultaneous origin of cholera at the north and south sides of Dublin, and for its early assault upon an isolated and well protected prison, whilst it took from three weeks to three months before it visited other prisons, surrounded on all sides by the disease, and in free communication with an *infected* public? How is it, more especially that the City Marshalsea, the worst and most exposed of all, was the last attacked? and how, let me ask, did it come to pass that a disease, affirmed and believed to be so highly contagious, that the dead must be *instantly* buried, should have evinced such mercy towards the inmates of the Dublin prisons, as to have attacked only five in Newgate, two in the Sheriff's Prison, four in the Penitentiary, and as many in the City Marshalsea, without enabling them or the ten in the Richmond Bridewell to furnish any proof of their contagious powers? How too came it to pass, that in two scholastic institutions, in the east and west of the city, (the Feinaiglian Seminary and the King's Hospital), with

* The contagionist may fairly be asked to explain upon what feasible principle he will account for the very tardy appearance of cholera in certain districts, between which, and infected places, there existed the most perfect freedom of intercourse. Thus, though cholera broke out in Belfast and in Dublin in March, the whole county Cavan remained free till the December following.

which I am professionally connected, and in neither of which any special precautions were adopted, not a single case of the disease occurred amongst about 300 individuals, though it raged most fatally for months in the immediate vicinity of both? * How came it to pass that the town of Finglas and its vicinage, situate on the high northern road, about three miles from Dublin, and containing about 1000 inhabitants, did not furnish a single case of cholera for more than three months after it had ravaged both Dublin and Drogheda, and with both of which places it maintained a constant and uninterrupted intercourse, but particularly with the metropolis?—To the contagionist I leave it to solve these queries, consistently with the doctrine so universally promulgated, and as the facts above referred to all came within my knowledge, I shall now appeal to others, which may be deemed quite as authentic.

Besides some minor parochial establishments for receiving cholera patients in Dublin, there were two extensive buildings appropriated to that object, one of which, (an old recruiting depot), was from its structure and very crowded state, admirably calculated for concentrating, as well as diffusing contagion, did any exist—the rooms were small, low, and ill ventilated; they were extremely crowded, and from the great pressure on the hospital, could not be kept as clean as might be wished. There were upon an average about 150 patients in the house, nine medical attendants, and above 100 nurses and servants; there was no separate accommodation or place of sleeping for the jaded nurses, who frequently threw themselves on the beds with the sick and dying patients, and there snatched a hurried sleep: under the severity of the pressure, a large proportion of them, as well as the medical officers, were incessantly in the wards day and night in

* Had strict precautionary measures been adopted in these instances, (as originally I had intended), what an argument would they have furnished of the influence of quarantine against the diffusion of cholera.

the freest intercourse with the sick, of whom upwards of 3000 were accommodated in four months. And yet, what was the result, and what were the proofs furnished of the contagious powers of the disease? Not a single medical officer was infected, nor more than two deaths amongst the nurses or servants, one of which cases will be more particularly specified. This detail was communicated to me by my young friend, Dr. N. Alcock, the senior medical assistant of the hospital, and will, I presume, be also authenticated by Mr. Hart, its respected superintendant, whenever they give to the public the valuable statistical and medical information they must possess of this disease.

The still more extensive hospital in Grange Gorman-lane, under Mr. Lindsay, furnished, as I have been given to understand, nearly similar results, some of which were published in the last number of this Journal. What then should be the conclusion from the foregoing details? Is it that cholera is possessed of contagious powers? if so, these powers must be weak indeed, and scarcely deserving of the notice and alarm they were made to excite. Shall I draw the conclusion, that the disease is propagated independently of contagion *in all cases*? However justified I might feel myself, by my own limited experience, in drawing such a conclusion, I do not venture to do so, until I am possessed of a larger mass of evidence on this subject, more especially as I do not find it easy satisfactorily to explain or account for some particular facts within my knowledge: for example, the case of the laundress referred to, who, after great fatigue, threw herself on a large bundle of foul hospital clothes, and having slept thereon, was seized the next day with cholera, and speedily died; this fact, however, or rather the inference from it, is opposed by the perfect impunity with which the nurses slept on the very beds with the sick and dying. I have been made acquainted with another curious fact, on such authority as enforces my belief: the first person who died at Finglas of cholera, was, previously to interment, carried round the parish cross and the church-yard three times, according to superstitious

usage ; the four men who carried the coffin on this occasion, all suffered immediately after from the disease and died,* and yet, it may be asked, how few have similarly suffered.

Having thus stated such facts as are calculated to throw some light on the supposed contagious properties of cholera, I propose resuming this subject hereafter, and instituting a comparison between it and the celebrated influenza of 1782 and 1803, as to their mode of propagation, and the remarkable identity existing between them in one respect at least, to wit, that the medical world was as divided in opinion regarding the contagious nature of the influenza as of cholera.

I shall now therefore draw this paper to a close, and in resuming the subject will give a short sketch of the two very different forms in which cholera appeared in the prisons, and of the very active and successful measures adopted for the cure of that which existed in the Bridewell, and which (according to the correct phraseology of my friend Dr. Alcock) may be designated the sthenic form of the disease.

* The following authentic return was made to me by Mr. Bourke :

“ Mary Neile died the 9th July, 1832, of cholera, and was buried the 10th ; Charles Dolan, Michael Sullivan, Owen Neile, and James Rooney, carried the coffin, and all died of cholera.

“ Charles Dolan was taken ill at the burial, and died next day at 10 o'clock, at his residence in Finglas ; Sullivan at 5 o'clock, evening of the 11th, and died in hospital next day ; Rooney taken ill same evening, and died in hospital two days after ; Neile taken ill same evening, and died in hospital three days after.

“ E. BOURKE,

“ Parish Clerk, and agent to the Officers of Health.”

BIBLIOGRAPHIC NOTICES.

The Principles of Surgery. By JAMES SYME, F. R. S. E., Fellow of the Royal Colleges of Surgeons in London and Edinburgh, Surgeon to the Edinburgh Surgical Hospital, and Lecturer on Surgery. Edinburgh, Maclachlan and Stewart, 1832, 8vo. pp. 716.

THERE must be something in the study of surgery not very congenial to literature, at least to that portion of it which teaches a concise or happy expression of idea. Within the last fifty years the world has been absolutely inundated with professional *books*, and if we take from our shelves the writings of Pott, Abernethy, and a very few others, we will allow any man to open one of the remaining volumes on chance, for the purpose of illustrating our remark. Even in more modern days, those who are enabled to cull original ideas from the *Memoires de l'Academie Royale*, or the *Dictionnaire des Sciences Medicales*, or the still more happy few who can peep into the recesses of German lore, seem to have devoted so much attention to foreign tongues, as to have occasioned a neglect of their own, and scarcely possess the merit of being good translators. One reason for this carelessness of style is, that we write for the present day, not for posterity; and if an author's reputation enables him to fill his pockets, he cares little for the glories of a literary immortality.

Another miserable failing of professional authorship is, the practice of substituting names for things, or rather for nothings, and thus cloaking a paucity, or a confusion of ideas, under an appearance of science. Alas! for the shade of poor John Hunter, with his actions, and his powers, and dispositions, words to which he, and he alone, could attach any meaning, and which, as they have served to puzzle the student since his day, have been diligently adopted by every professional writer, who had a desire to display his knowledge. We recollect an eminent and worthy surgeon to have been once examined as a witness in a case, where a nurse brought an action against the father of a child, from whom she had contracted venereal. Neither of the parents seemed to suffer from a single symptom, and he was called on to shew how, under such circumstances, the infant could possibly exhibit such a mass of disease. Unfortunately,

in his explanation, he stumbled on the words "latent disposition," which he was asked to render into plain English, for the benefit of the bench and of the bar, and here he got himself into the middle of a morass ; away he floundered, still sinking deeper and deeper at every attempt to extricate himself, until he was left over head and ears, amidst the sneers and laughter of the long-robed gentry, cursing the hour in which he had first listened to such unintelligible jargon.

With feelings of the warmest compassion for the student who is obliged to undertake the study of a profession loaded with bad Latin and barbarous names ; we heard with delight the announcement of such a book as the "Principles of Surgery" by such a man as Mr. Syme. We had known this gentleman as a teacher, an operator, and a writer, and we thought him fully qualified to sweep away the mass of rubbish with which the very threshold of the profession is encumbered, and lay down, in plain unvarnished terms, the simple facts on which the entire superstructure is afterwards to be raised. The principles of any science should be, like the axioms in Euclid, propositions either self-evident, or capable of the simplest and clearest demonstration. Unfortunately this is not the case with surgery, for we find the very elements of the science, namely, the doctrine of inflammation, so distorted by theories, and so disfigured by false reasoning and fallacious experiments, that we defy any student to arise from the bench of a lecture-room, or from the chair in his closet, where he had been seeking for information on this subject, with any sensation but that of utter and almost hopeless dismay, at the difficulties that beset him even in the commencement of his career.

Aware, then, of the imperfections of the present mode of teaching, and of the absolute necessity that exists for the introduction of some other method ; knowing that there is no lack of elementary books constructed on the present system, and having heard that Mr. Syme intended his volume to be a kind of explanatory vade-mecum to the students of his own class ; we deemed it highly probable that the forthcoming volume was to be the desideratum we had so long looked for. How far our expectations have been realized will be seen in the sequel.

The part of the work devoted to Inflammation, occupies a little more than 80 pages of a book, printed in a large type, the lines widely spaced, and with a very respectable and cleanly-looking breadth of margin ; including Inflammation, its causes and its treatment, Mortification, Effusion, Absorption, Granulation, Ulcers, and Suppuration. Such is the order of arrangement observed. By the way, we may as well mention here *en parenthese*, that erysipelas can no longer be considered as a form

or species of inflammation, for the word even never occurs in these 80 or 85 pages : indeed we may suppose that in the sister island this disease is no longer submitted to the management of the surgeon, for it forms no part of the work before us, except merely as a cutaneous affection, where a few words are allotted to it. The "Nature of Inflammation," or as it was formerly termed, its proximate cause, is begun, continued, and fairly disposed of within the compass of nine pages, and in these, the old doctrine of derivation and revulsion, the error loci of Boerhaave, the spasm of the extreme vessels of Cullen, the increased action of the vessels, with their action of dilatation of Hunter, the debility of Vacca, the microscopic experiments of Wilson Philip, these, and all these, are suitably commented on and discussed. And at length, what is the definition of inflammation at which we arrive ? It is printed in Italics, "*a perverted action of the capillary system, generally attended with heat, pain, redness, and swelling !*" If any student can be so stupid, as not to understand what the nature of inflammation is, after this simple and accurate definition, we cannot help him ; it is neither our fault nor our author's.

The symptoms of inflammation are described succinctly, but correctly enough, and we therefore pass them, for it is not our intention to give an analysis of the work, but merely an opinion of its merits, and proceed to make a few observations on the chapter devoted to its treatment. And here we must remark on the want of any thing like arrangement that prevails in this department : there is no separation of local and constitutional treatment, no distinction drawn between the different stages of the affection, and the remedies applicable to each, no notice of the influence of constitution on the disease, and scarcely any even of the symptomatic fever. Under the head of "Diaphoretics" we read that "local baths are of great service in the form of fomentations and poultices ;" and under that of "astringents," we are told that "cold acetate of lead and nitrate of silver are the best means of the kind," and thus the nature of topical applications, and the comparative value of heat and cold, are all dismissed with the information contained in the foregoing words. Let us look to the general summary of all. "General bleeding is of no use as a preventive of inflammation, *unless it removes some derangement of the system or counteracts its tendency to excite inflammation.*" Read the words thus marked in Italics, and the sentence is nearly this, Bleeding is of no use as a preventive of inflammation, unless it prevents it. Again, in the next sentence, "It" (i. e. bleeding) "has most effect at the commencement of inflammation, and is most beneficial when there is great *power of action.*" Again and again we protest against this kind

of language, because we insist, that to the majority of students, aye, and to some practitioners too, it is perfectly unintelligible. We always understood that in every living body each part performed certain functions ministering to the well-being of the whole, and the process by which such function was completed, John Hunter chose to call "the action of the part." Now this action having a tendency to impair, or to use a more familiar expression, to wear out the part, it became necessary to counter-balance it by some faculty of resistance, or perhaps, more properly, of endurance, also derived from the vital principle, and this he termed "the power of the part." Placing these terms thus in opposition, it may be barely possible to attach an idea to each, but when a teacher begins to speak of the "power of action" in an elementary book, it evidently tends to produce such a confusion of ideas as would be sufficient to disgust the most persevering student. The same kind of artificial language pervades the entire article: we have "an excessive disposition to act," as one of the causes of mortification; "the gangrenous or dying action extending itself over the system," as explanatory of the peculiar constitutional symptoms that attend the death of a part. But enough of this; if Mr. Syme's mode of expression be faulty, it is that which has been used so long and so commonly by professional writers, that he can scarcely be blamed for not having deviated from the beaten track.

But the chief objection we have to the work before us is its size. As a syllabus of a course of lectures it is infinitely too large and too expensive; as a book of explanation it is too meagre and insufficient. As a companion to the student going round the wards of an hospital, it is perhaps less useful than Martinet's little compendium of Pathology: as an introduction to the knowledge of the principles of surgery, we deem it scarcely equal to Cooper's first lines. In a word, the book to be useful, should have contained more, it should have entered more into detail, and treated of its subjects more amply. There are not, throughout, many errors, or many fallacious opinions, but there is a deficiency of argument, illustration, and proof; and whilst we acknowledge the value of the principles therein laid down, it is not from any force of demonstration that attends them, but because we have derived a knowledge of their truth from other sources.

But our author is an hospital-surgeon, and an operator, we shall therefore come at once to some points of practice with which, of necessity, he must be familiar. Passing over the remainder of inflammation, and the effects of diseased nutrition, tumours, &c., we come to the diseases and accidents of blood vessels, subjects confessedly of the greatest importance, and on

which every British surgeon should possess the best information, inasmuch, as more has been done in the British islands to extend our knowledge on these branches, than in the rest of the civilized world.

The section on aneurism is comprised within thirteen of our author's short pages, in which space, we are not to be understood to say, that each particular aneurism is described, but only that it contains the history, nature, and symptoms of the disease, the different modes of treatment, and the principles on which the cure is effected, so far as it seemed good to him to explain them. The division of aneurism is not that usually employed by us, but is adopted from the French; it is divided into the true and false, but besides the dilatation of the artery, that which we hitherto called a circumscribed aneurism, is here denominated a true one, whilst our diffused aneurism, (if indeed it is a diffused one that is meant), is here termed false. On this point we are not perfectly certain, and, therefore, we must quote the very words.

“In false aneurisms, none of the coats remain entire, and *the sac* is formed, in part of its extent at least, merely by the surrounding cellular substance. They are generally of larger size, and more irregular figure, than those which retain part of the arterial coats in their composition, whence also their contents are usually more completely coagulated. They are produced in two ways: 1. By the formation at once of a breach through all the coats of the vessel together, which may be effected either by violence or ulcerative absorption: 2. By the sac of a true aneurism giving way, so as to allow its contents to escape and distend the cellular substance *into another bag*.”

From the general context of this passage, we believe, that it is the diffused aneurism that is meant, and if it is not, there is no such thing as a diffused aneurism even hinted at in the entire work. We used to think that the great difference between a circumscribed and a diffused aneurism was, that in the one, the effused blood was contained in a cyst or sac, whilst in the other it was poured out into the cellular tissue, without any such limit to its extension. Now it appears that they both have sacs, only that one is larger than the other. Such, at least, is the legitimate inference to be drawn from the above quotation, if by the term false, a diffused aneurism is meant, but of this we are not quite certain.

In order to show our author the fairest play, we must still continue our quotations.

“The natural course of aneurism is to grow larger and larger, to change from the constitution of the true, into that of the false kind,

and then to terminate in one of the following modes. 1. The contents coagulate, and are absorbed with, or without obliteration of the vessel. 2. The artery becomes obstructed from coagulation or the effusion of lymph, and ulcerative absorption of the parietes of the tumour allows its contents to escape. 3. The sac opens by ulceration or sloughing, without previous obstruction of the vessel. The consequence of the two first of these terminations is a natural cure, that of the third a fatal hæmorrhage.

“The treatment of aneurism consists in the use of means, which tend to promote one or other of the salutary processes just mentioned. The earliest attempts of this kind were directed with a view to the second of them, and their rudeness was equalled by their severity. The sac having been opened by cutting or burning, its contents were turned out, and then the hæmorrhage was restrained by repeated applications of the actual cautery, or some other powerful styptic. The introduction of the ligature for closing the mouths of arteries rendered this operation less painful, formidable, and uncertain, but the difficulty and danger attending it, continued to be very great, and the instances of recovery were extremely rare. Surgeons, therefore, turned their attention to the other mode of natural cure, and endeavoured to promote coagulation, by lessening the force of the circulation through the effect of bleeding, low diet, and rest. This plan of treatment is usually distinguished, as that of Valsalva, who was its principal supporter. In addition to these means, Guattani recommended tight bandaging of the limb and tumour. Both methods were found to be extremely inefficient and uncertain, so that though productive of less harm, they hardly afforded more benefit than the bloody proceeding which has been already described, and was in those days called the operation for aneurism. The good old fashion of performing amputation of the limb affected, was, therefore, generally followed, until it fortunately occurred to John Hunter, (1785), and much about the same time to Desault, that an effectual method of causing coagulation, would be, to obstruct the artery above the tumour, or between it and the heart, so that thus the natural cure by coagulation and absorption, might be safely and certainly induced.”

Now this is all very true, but it is not enough. It does not contain “the principles of the surgery of aneurism.” We ought to be told, that pressure on the wounded spot in the vessel, to the extent of obliterating it, is the principle on which the entire cure depends, and that wherever it would be either injurious or impracticable to effect this pressure, it should not be attempted at all, but the limbs should be removed at once. In favorable cases we can apply this pressure from without, by means of compress and bandage, many instances of the success of which practice have lately occurred in Steevens’ Hospital, and have been published in this Journal. On the same principle we apply a ligature on the vessel be-

tween the aneurism and the heart, that the impulse of the heart being removed, the blood within the sac may have time firmly to coagulate, and effect the requisite degree of pressure. From this one principle we should be taught to deduce the reason, why a ligature at a distance will not cure an aneurism, accompanied by a wound that communicates with the interior of the sac. Why the cure becomes uncertain when the pulsation returns in the tumour, and in what instances the re-appearance of this symptom proves the operation to have totally failed. We might then be told the cases in which the attempt should not be made, such as a diseased condition of the arterial system, inasmuch as we do but little good by controlling an internal hæmorrhage, which an aneurism is, if in doing so we produce an external one, in the form of secondary bleeding; or the aneurism being of such size, as that its contents could not be absorbed; or the general diseased condition of the limb; or a number of other circumstances which would make us abandon the principle of pressure, and resort to amputation, "that good old fashion" as it is called.

The subject of secondary hæmorrhage belongs not to the principles of the surgery of aneurism, but to the pathology of the arterial system, inasmuch, as it may occur after any accident or operation, in which an artery has been tied. It occurs, however, sufficiently often, after the Hunterian operation, to warrant its being brought forward with it, and we shall just notice our author's remarks upon the subject.

"The hæmorrhage usually occurs about the time when the ligature separates, which is generally from the fourteenth to the twentieth day after the operation; but it may likewise occur either much sooner or considerably later. It appears for the most part very insidiously, not exceeding a few drops, but recurs from time to time in increasing quantity, until the patient, after being repeatedly saved by syncope, is finally exhausted. Pressure, together with the means which tend to lessen the force of the circulation, may be tried in the first instance; but if the bleeding continues, or returns, the surgeon must, without delay, either tie the artery nearer the heart, or amputate the limb."

In the first place, the hæmorrhage does not take place at the time when the ligature ought to separate healthily and safely, but much earlier. This period is different in different arteries, according to their size. The ligature from a subclavian artery will scarcely come away before the 23d day; that from the carotid about the 19th; from the iliac about the same period, and from the femoral about the fifteenth. Now, secondary hæmorrhage takes place from the latter vessel generally about the ninth day, and so on, with the others in proportion,

so that it becomes a principle of surgery, that the longer the ligature remains undetached, the safer the patient, and the operation more likely to prove successful. Again, it does not appear insidiously, but is generally ushered in by a train of symptoms, known under the name of hæmorrhagic fever, by every surgeon that ever had the misfortune of attending such a case. Lastly, we object to the last sentence of the passage, because, instead of placing the awful reality before the student's eyes, instead of telling him that not more than one in every ten recovers, and that one having undergone the dangers of bleeding, or of inflammation and gangrene, resulting from the measures resorted to control it, he is rather led to infer, that if pressure does not succeed, he has still a certain resource in tying the artery higher up, or in amputation. But he has not, if the femoral artery bleeds on the 9th day, and is taken up nearer the groin; it will bleed again in this new situation on the 3d or 4th, and if the external iliac be tied now, it will bleed within 48 hours. This, certainly, has not happened in *every case*, but nevertheless, is so frequent an occurrence, that, in our minds, it has become "a principle of surgery," that an artery so circumstanced, *ought not* to be tied nearer the heart. As for amputation, it does not succeed, and we do not believe it could. "Pressure, therefore, together with the means which tend to lessen the force of the circulation," are the only measures which encourage a rational hope, and heaven knows, but a very slender one.

Having offered these few remarks, which have been suggested by a wish to remove any erroneous impression likely to be occasioned by a book, which we suppose will meet a very extensive circulation, we shall open the volume in another page, our object being, not to afford our readers an analysis of the work, but rather to shew them in what manner it has been executed. The first fourteen chapters treat of what may be termed general subjects, injuries, and diseases of organs and structures, arteries, veins, bones, joints, tendons, &c., without reference to any particular situation in the body: the fifteenth commences with wounds of the throat, and is followed by others treating of the thorax, abdomen, pelvis, genital organs, brain, &c., thus arranging injuries, diseases, and operations, with reference to their locality. The arrangement is good. It first supplies facts and principles derived from, and depending on, peculiarity of structure, and then shews their application, with such modifications as must result from situation and function. As a general observation, we may be permitted to say, that we like the execution of this second part much better than the

first, although still there are grounds for complaint in the want of detail and illustration, and in the hurried manner with which important subjects are disposed of. For instance, in the very chapter before us, we find wounds of the throat, œdema glottidis, ulceration of the larynx, croup, foreign bodies in the larynx and trachea, laryngotomy, and tracheotomy, all treated of in the space of eight pages and a half: the removal of foreign bodies from the pharynx and œsophagus, œsophagotomy, stricture in the œsophagus and bronchocele, with the different operations proposed for its relief, in twelve pages more. Assuredly, the importance of these subjects is such, as to demand, that they should not be thus rapidly, we had almost said slightly, passed over, for independently of the agonized sufferings of a patient placed in such circumstances, which charity should call upon us promptly to relieve, it cannot be too firmly impressed on the student's mind, that the decision of a moment may make all the difference between life and death to the patient. We, therefore, insist that it is not, and ought not to be sufficient, to enumerate a certain number of diseases, for the relief of which, any given operation might be attempted; but a line of demarcation should be drawn between those in which it may be available, and those in which it would be worse than useless; between those in which some delay may be permitted, and those in which the loss of a few minutes may lead to inevitable destruction. Without such knowledge, and without such capability of establishing a diagnosis, no man need hope to acquire the reputation of an accomplished surgeon, no matter what dexterity of hand he may possess.

Let us apply these remarks to the work before us. In the first instance, the different situations in which the throat is cut by the suicide are mentioned, and the fact of the usual escape of the large blood vessels noticed: but there is not a single symptom detailed, unless we take the following passage to be descriptive of all the phenomena, both local and constitutional, and of all the agony, both mental and corporeal, which such a miserable being must endure.

“The medical treatment of cut-throat is always extremely important. There is generally great cerebral excitement; the desire of self-destruction frequently continues in operation, and the profuse hæmorrhage which sometimes happens, exposes the patient to the danger of excessive re-action. It should be recollected also, that the irritation of the wound may occasion swelling of the pharynx, and other deep-seated parts of the throat, very adverse to free respiration, and may lead to bronchitis, more or less acute. Every source of bodily or mental exertion must, therefore, be strictly guarded against, while cold, counter-irritation, opiates, and antimony, are employed

according to circumstances. And if it should seem, that the air does not obtain a sufficiently free entrance, an opening into the trachea must be effected without delay."

All this is true, but it is not enough. It may answer very well for the educated practitioner, who is already acquainted with the phenomena of "cerebral excitement," and with the consequences of "bronchitis," but what is the young student of a few month's standing to say to it, when he takes up this book in the hope of finding therein the very first elements of his profession? It is not very probable that he will find these remarks perfectly clear and satisfactory, coming in (as they do) at the very termination of the article, and after other modes of treatment, such as the introduction of tubes through the nares into the trachea and œsophagus, have been discussed. By the way, we may here be permitted to remark, and it is an observation furnished rather by the entire work, than applicable to any particular passage, that the operations of surgery must be far more easy of performance, and far more fortunate in their results in Mr. Syme's hands, than they are in ours on this side of the channel. Of every point of practice, *il parle fort à son aise*, as a Frenchman would say. We have already noticed this with reference to secondary hæmorrhage; and here again, there is not a single syllable to impress on the student's mind the awful nature of his patient's condition. In certain circumstances we are directed to introduce a tube into the œsophagus, which "may be passed either by the nose or mouth," and its introduction is acknowledged to be extremely difficult: but nothing is said of its still more frequent impossibility; nothing of the irritation occasioned by awkward attempts at its introduction, which produce consequences far more injurious than the tube could prove serviceable. We are left ignorant of the fact, that often it is a matter of no small difficulty, to ascertain whether a tube has passed into the trachea, or the œsophagus, even with the assistance of a lighted taper held before its aperture, and, finally, that no patient will endure the pressure of the tube for more than a few hours. As a means of assisting his pupils in forming a just prognosis, he should have mentioned the proportion of recoveries he has met with amongst cases of attempted suicide; with us it has been miserably small indeed.

But we have done with these remarks, which some will think have been drawn to an unnecessary length, and others to have been introduced in a spirit of unfair and uncandid criticism, inasmuch as our author, in his preface, expressly declares his work to be intended as a text-book for surgical lectures, and acknowledges it to have been, on many accounts, not in his power to

enter very minutely into the various subjects treated of, referring to the Surgical Dictionary, and other works, those who might require more particular information. We are not disposed to quarrel with any author who lays his pretensions thus modestly before us, neither shall we find fault with so strange a misnomer, as the calling a syllabus or text-book by the name of the Principles of Surgery, but it being evident that we have not been perfectly satisfied in the present instance, we must endeavour to justify our opinion on fair and honourable grounds. In order to do so, we must, in the first instance, explain our own ideas of what a lecture should consist of.

In teaching any profession so extensive as that of surgery, we must suppose the professor to attach more importance to one subject than another, and therefore to bestow on it more time, labour, and attention. Such, it may be easily conceived, he will have the greatest pleasure in explaining, and his demonstrations will afford the greatest satisfaction, whilst of the others he may be excused for being comparatively careless, but great care should be taken in the selection, and those diseases or accidents directly involving human life, should never be lightly taken notice of. Again, we conceive that the least satisfactory description of any disease must embrace the following heads, viz., its symptoms, or the circumstances, indicating either to the senses or to the understanding the existence of such, or of some similar affection; its diagnosis, or the signs that establish its identity, and separate it from all others bearing its resemblance; its pathology, or those derangements of function or of structure which either precede or accompany it; its treatment, including, of course, all surgical operations, and, lastly, its prognosis, or the opinion we may be authorized to form of its ultimate termination. This latter is of vast importance, for independently of the anxiety a patient or his friends must feel as to the future, it involves the necessity of a thorough acquaintance with the nature of the disease, its severity, the importance of the organ engaged, together with calculations founded on the constitution of the patient, and the results of our experience and that of others in similar cases. Now, although we consider all these features to be of importance in the proper delineation of any disease, and should pronounce any text-book to be deficient in detail, and unequal in its execution, wherein any of them should be neglected or omitted, yet viewed as the constituent parts of a lecture, there are some of them to which a greater degree of consequence should be attached. These are the diagnosis and the prognosis of disease. It is on these subjects that the observations of experience are truly invaluable, and confer professional superiority on their possessors. Any man can cure a disease (it is

said) if he knows what it is, and surely he must best know what it is who can distinguish it from all others, and confidently predict its results. Let us see then, in a few instances, (and we shall cull them at random), how far this elementary work answers our expectations; we have already stated that we impeach not its accuracy, we only lament its insufficiency, even within that limited sphere professedly assigned to it.

The description of œdema glottidis is wholly omitted, inasmuch as it "is now so well known to the profession, that any account of its symptoms or progress would here be equally unnecessary and out of place." The possibility of a morsel being stopped in the pharynx, and by compressing the epiglottis, causing sudden death, is not noticed, although it is one of the cases in which bronchotomy has been most successful. The effects of swallowing boiling water and the concentrated acids, and the resources which surgery offers, are wholly omitted. There is, in our opinion, an erroneous idea inculcated of the manner in which a foreign body finds its way into the wind-pipe, where it is stated "that it generally happens in the act of deglutition;" and, finally, there is no impressive notion of the danger of any of these affections, nor even of bronchotomy considered *per se* as an operation, sufficiently conveyed.

When we come to the thorax, we find the same mischievous doctrine that has so long embarrassed our pathological investigations of injuries of the important cavity, arising from an idea that the lung is merely a passive organ, not endowed with any vital action at all, and resembling a bladder within a pair of bellows, to which it has been more than once compared. In wounds of the thorax, "if air be admitted into the aperture, as must almost necessarily be the case, the lung of the side affected inevitably contracts itself, owing to the *elasticity of the pulmonary tissue* being thus placed in equilibrio as to the pressure of the atmosphere. If only a small quantity of air has been allowed to enter before the wound is closed, the contraction of the lung will be proportionably inconsiderable. But if the wound remains open until the contractile tendency of the lung exerts its full effect, respiration, so far as that half of the pulmonary apparatus is concerned, will be completely suspended." Will Mr. Syme tell us how it happens, that a man stabbed in both sides of the chest, and lying long enough on the field of battle for the *elasticity* of both lungs to exert its full powers of *contraction*, should ever recover; or does he deny the possibility of such an occurrence? Again, "*when the lung is wounded it must suffer a complete contraction*, since the air, though it should not be allowed to enter by the external wound in sufficient quantity to occupy the space resulting from the entire collapse, will find a

ready entrance from the branches of the trachea which are wounded ; and the external surface of the lung being thus maintained permanently in equilibrio with the internal, as to the pressure of the atmosphere, the elasticity of the organ will meet with no obstacle to the exercise of its contractile tendency." Now this pathology is, however plausible, all wrong. First, elasticity is merely a dead force, and to allow of its being exerted in the present instance, we must suppose that in a state of health the lung is kept in an unnatural state of tension : secondly, there is no proof of the structure of the lung being elastic, independently of the air it may contain, for no one ever demonstrated elasticity in the lung of an unborn infant : thirdly, there is not and cannot be a *collapse* of the lung, for no mechanical force, short of what will disorganize and destroy it, can expel all the air even from a dead lung : and, lastly, it is practically incorrect, inasmuch as many have suffered from wounds of both lungs and recovered notwithstanding. We must own, we like not that pathology which places an organ of such vital importance, and probably endowed with such vital energies, on a level with dead matter, likely, as John Bell expresses it, to collapse and fall down against the spine like a torn bladder.

In the enumeration of affections of the thorax there is a wonderful omission ; the word *empyema* never occurs throughout the entire article.

The operation of *paracentesis thoracis* is well described, as most of Mr. Syme's operations are, but not a syllable is there to inform us of the cases to which it is applicable, nor to enable us to form a diagnosis among the numerous diseases of the chest, which must of necessity bear a strong resemblance to each other. Lastly, the student is left to guess at the results of his operation, or rather he is indirectly led to suppose it free from danger, for he is never informed how few, how very few recover.

We must now conclude, having extended these observations far beyond our original intention, but we felt obliged, as we could not offer unqualified approbation, to enter into some lengthened details of the reasons that induced us to withhold it. Mr. Syme's name stands most deservedly high in the profession, and there cannot be the least doubt of his capability of producing an admirable work of the nature of that before us, provided he had only adopted some better mode of arrangement, and bestowed on it a little more care and consideration. Our chief objection to his book is, that there is not enough of it. For its size and its price, its contents should have been at least doubled. It is a great and crying evil, that the prices of professional books are such as actually to place them beyond the reach of those who most stand in need of them, namely, the student and the

young practitioner, and when this expense is unnecessarily increased by blank pages, broad margins, and spaced lines, it is only fair to inquire whether these vacancies might not have been advantageously filled up. In this spirit we have commenced, and in the same we shall continue, until we succeed in pulling down and destroying the entire system of *book-making*; the high rank and great respectability of our author, on the present occasion, being a certain guarantee that minor offenders shall not escape.

New Views of the Process of Defecation, and their Application to the Pathology and Treatment of Diseases of the Stomach, Bowels, and other Organs; together with an Analytical Correction of Sir Charles Bell's Views respecting the Nerves of the Face. By JAMES O'BEIRNE, M.D., Surgeon Extraordinary to the King, one of the Surgeons of the Richmond Surgical Hospital, Dublin, &c. &c.

WE are greatly gratified when we meet any thing original. Our readers, for whom we wish to cull whatever we meet worth gathering for its goodness or its rarity, can not conceive the irksomeness of our task as reviewers, in conning over what are called new books, where in most cases, we meet again, what had been before too often repeated, only differing in this, that like a badly reflected image, the repetition is not half as good as its original. In poring over such books, we cannot avoid nodding at our task, and perhaps imparting to our readers some of the contagious drowsiness. But we are aroused from our lethargy by genuine originality, and spring upon any work that will repay us for the exertion, with as much avidity as the hawk that seems listlessly sleeping on the air, will turn and pounce upon his proper prey. A book of original ideas, or discoveries, gives us, by the stimulus of its novelty, a fresh impulse for exertion, and our readers an increasing inducement to accompany us. That originality we have found in the pages before us, and we hasten to share it with our readers. Dr. O'Beirne has undertaken the perilous task of overturning one of the most universally received, and undisputed parts of the physiology of the digestive organs. The question at issue, between Dr. O'Beirne on the one side, and all preceding authors on the other, is, moreover, not one of mere abstract physiology, but involves in its results, principles and consequences of the greatest importance in practical medicine. We

may with advantage consider the work under the three heads of physiology, pathology, and practice. From the title of the book it will be easily surmised, that the functions of the large intestines are the more immediate objects of examination, and our author asserts, that all the previously received notions on their functions are erroneous. We shall make the matter more clear, by first bringing to our readers' recollection, what those received opinions are, as taught in standard works on physiology, and then contrast with them Dr. O'Beirne's views. The description, as hitherto taught, of the functions of the large intestines, usually runs thus:

"The excrements thicken, harden, and become moulded in the cells of the colon, they are then urged by the peristaltic motion into the rectum, *in the cavity of which they accumulate*, till they excite on its parietes an action, which determines their expulsion, &c. The combined action of the rectum, and of the abdominal muscles, overcomes the resistance of the sphincters, &c."—*Vid. Richerand, &c.*

In the above extract the rectum is described as the receptacle for holding the constantly accumulating feces, and as the antagonist of the sphincter muscles of the anus. Dr. O'Beirne asserts, that the rectum, unless in a morbid state, is never a receptacle for feces, and that, instead of its being an antagonist of the sphincter muscles, its office is in common with those muscles, to keep the lower portion of the intestinal canal closed, until the call for expulsion overcome both the action of this muscle, and of the sphincters. We shall let our author speak for himself, first giving the new positions and discoveries which he advocates, and then the grounds on which he supports his opinions.

"—they distinctly prove, first, that in the healthy and natural state, all that part of the rectum above its pouch, is at all times, with the single exception of a few minutes previous to the evacuation of the bowels, firmly contracted, and perfectly empty, at the same time, that the pouch itself, and also the sigmoid flexure of the colon, are always more or less open and pervious; lastly, that the sphincter ani muscles are merely subsidiary agents in retaining the feces."—p. 8.

We shall now give some of the grounds and proofs by which Dr. O'Beirne supports the above positions. He first observes, that if the rectum were (as we have hitherto been taught to believe) a receptacle for feces, the accumulation in it of excremental matter would necessarily interfere with the functions of the bladder, and would also be a constant source of irritation to the sphincter ani muscles, and thus deprive the individual of the power of retaining his feces. From the very great resistance frequently given to the passage of an enema, he also draws an argument

in support of his position, the resistance proving, "that the rectum, so far from being open, is firmly contracted and closed." The next proof is of great weight.

"Surgeons find it necessary to pass a finger up the rectum, either to direct the course of a catheter, sound, or staff, to discover whether a fistula communicates or not with the bowel; to detect the presence of a calculus in the bladder, or a stricture in the intestine itself; to ascertain the state of the prostate gland; and for various other purposes; and yet it is a fact that it has exceedingly rarely happened, that, on any of these occasions, the finger has encountered either solid or fluid feces in the rectum, or presented a soiled appearance when withdrawn. Indeed, as far as my experience and inquiries enable me to speak on the point, in the few instances in which such examinations have detected the presence of excrement in the healthy rectum, it has been invariably found in very small quantity, and never in any but the lowest part, or pouch, of this intestine. It is, also, a fact familiar to apothecaries and nurses, that the pipe of the injecting syringe, however long it may be, is rarely, if ever, found soiled with fecal matter when withdrawn after administering an enema. These circumstances show that the rectum is contracted and closed, so as to prevent free communication between it and the sigmoid flexure."—pp. 4, 5.

The arguments drawn from pathology are very ingenious and forcible.

"Membranous filaments have seldom, if ever, been found traversing in various directions the cavity of either the small intestines, the cæcum, or the colon, while they have often been met with in the rectum. This fact proves that the parietes of the rectum must have been contracted, and its lining membrane in close contact at all points, for a time sufficient to effect the firm organization of these filaments, and, consequently, that there could have been no communication between this intestine and the sigmoid flexure for, at least, several hours.

"The two sphincter muscles of the anus are considerably weakened in the disease called prolapsus ani;—in the operation for fistula in ano, these muscles are completely divided, and thereby wholly incapacitated, for a certain time, from acting as sphincters;—not only these muscles, but also a portion of the rectum above them, are occasionally destroyed by venereal, cancerous, and other ulcerative processes; yet it rarely happens that the power of retaining the alvine contents, is found to be at all impaired in any one of these cases. It is therefore manifest that this could not possibly occur, if the passage into the rectum were as free as it is supposed to be, or if the power of retaining the feces, and regulating their discharge, depended solely on the sphincter muscles of the anus."

We may observe, that in the above we have a satisfactory

explanation of the power of retaining their feces, which women possess, who have had the recto-vaginal septum torn. Burns explains it on the supposition of a valve being formed by the torn part; his explanation is very defective compared with that which can be now offered. Dr. O'Beirne gives the results of his examination of the rectums of healthy persons made with the stomach tube, and deduces from them inferences in support of his views. For these we must refer our readers to the work itself, as also for the observations on the nervous influence supplied to the rectum, on its being the only part of the intestinal canal receiving regular spinal nerves, and on the explanation hence drawn, of the effects of remedies administered by enema, and of various symptoms presented by the rectum in paralysis, irritability of the spinal column, &c.

We now come to our author's peculiar views of defecation, or "the process by which the contents are moved through the bowels, and finally, expelled from the body."

In the first part of this description as relating to the manner in which the food is passed as far as the ileo-cæcal valves, there is nothing different from what has been already taught. Our author denies that the ileo-cæcal valve can ever permit a retrograde motion of the feculent contents of the colon. The views relative to the sigmoid flexure of the colon and its action in defecation now come before us.

"—— the fecal mass has been conveyed into the sigmoid flexure, and that the repeated descent of similar masses causes this portion of the colon to become distended, and to ascend from the cavity of the pelvis into the left iliac fossa. When this occurs, the flexure, in proportion to the rapidity and degree of its distension, begins to turn upon the contracted rectum as upon a fixed point, until at length, like the stomach, it directs its greater arch forwards and upwards, and its lesser backwards and downwards. By this movement the contents are brought somewhat perpendicular to, and so as to bear directly upon the upper extremity or annulus of the contracted rectum, but as their mere weight is insufficient to force a passage downwards, and as this end cannot be accomplished, either by such gentle pressure as that exerted by the alternate contraction of the diaphragm and abdominal muscles in ordinary respiration, or by the efforts of the flexure itself, in consequence of its muscular power being so very inferior to that of the rectum, they are compelled to remain stationary, until such time as the increasing accumulation and distension produce a sense of uneasiness sufficient to call into action those great expulsive agents, the diaphragm and abdominal muscles. These muscles, instead of acting alternately, now act simultaneously, compress the abdomen and its contents on all sides, urge the free and floating mass of small intestines downwards, and even into the cavity of the pelvis,

so as to press forcibly upon, not only the distended sigmoid flexure, but also upon the cæcum and the urinary bladder. By these means the contents of the distended flexure are acted upon in every direction, and so as to be impelled against the upper extremity or annulus of the contracted rectum, with a force sufficient to compel the parietes of this intestine to separate and afford a free passage. The nismus now ceases, but as soon as the rectum becomes filled, it is roused to make an expulsive effort, by which the whole of its contents are driven and impacted into the pouch. Here their accumulation produces a great sense of weight and uneasiness in the perineum, an urgent desire to go to stool, and a still stronger nismus, by which the sphincters are forced open and dilated, and the final expulsion of the egesta is effected. But the urinary bladder, although it is subjected to considerable pressure during this process, is not evacuated at the same moment, but immediately after, because, during this the last stage of the process of defecation, the accumulation within the pouch and dilated sphincters, presses up the prostate gland against the arch of the os pubis, and thus effectually prevents the flow of urine, until the accumulation is removed. The evacuation of the rectum and bladder being completed, immediately the nismus ceases, the rectum and the sphincters return to their former state of contraction, the diaphragm reascends, carrying with it and restoring to their proper situations the liver, the stomach, the spleen, the small intestines, the cæcum, and the ascending, transverse and descending portions of the colon. But the inferior portion of the sigmoid flexure is differently situated. Having a remarkably long and free process of peritoneum, and being empty, it is compelled, during the last expulsive nismus, to occupy part of the space which the evacuation of the bladder and rectum leaves in the cavity of the pelvis, and must of necessity remain in this situation, until it becomes again distended, because, as a mere glance will show, the manner in which the peritoneum connects the small and large intestines with the diaphragm is such, that from the descending portion of the colon being bound down to the abdominal parietes, this is the only portion of the intestinal canal which does not follow, and is not in the least influenced by the action of the diaphragm. This is the fact which induced me to assume that the situation of the empty flexure in the living body, is the same as that in which it is uniformly found after death.”—pp. 17—23.

There are some further details connected with the description of this process, on which we have not space to enter, and in some of these details Dr. O’Beirne has fallen into the error of those who prove too much. He assumes that the colon always contains a proportion of gaseous matter; it is very likely that the colon generally does contain gaseous matter, but to build any part of the explanation of the process of defecation upon the supposition of the colon in its healthy state always containing air, is to build upon a weak foundation. The suppo-

sition is, however, not at all necessary to support Dr. O'Beirne's main views, and it would have been better omitted. Dr. O'Beirne, as a military surgeon, has seen much service in the field, and we believe, in military tactics, as in argument, that when a position is weak, and more particularly when not requisite, it is better to abandon than support it.

We have now gone through the first part, or what we proposed to consider under the head of physiology. We next arrive at pathology. If irritation be produced in the intestinal canal, it may produce diarrhoea, but if it be higher in degree, or act upon the rectum, its effects will be, according to Dr. O'Beirne's views, to increase what he considers the proper action of this intestine. "It will be stimulated to still greater contraction, and thus cause constipation," which lasting for some time, may distend the large intestines to such a pitch, as to stretch beyond their tolerance the laminae of mesentery surrounding the caecum, &c., and thus superadd a new disease, peritonitis. In the observations on stricture of the rectum, as connected with the functions of this intestine, our author supports the existence of spasmodic stricture of this gut, and enters the lists against Mr. Colles, who doubts of the existence of this disease. We must refer to the book itself for the discussion on this subject, which is practically interesting, and leave our readers to form their own opinion, as to which side the arguments incline. There next follows a very interesting article in the form of a note, on the pathology of organic stricture, its seat, &c. We have not room for it, but it will amply repay the reader for an attentive perusal. In the course of observations on pathology, Dr. O'Beirne adverts to impacted feces, lodged in the rectum, as a cause of constipation. He considers this cause as one rarely met with, and explains its occurrence, on the principle, that in such cases the rectum partakes of the general muscular debility of the aged, in whom, almost alone, this form of disease is met with, and thus permits the morbid accumulation to take place within its cavity. Our readers will remember what we described a little above as Dr. O'Beirne's view of constipation, that in general it is nothing more than "an effect of an unusually contracted and impervious condition of the rectum." The curative indication is of course "to mechanically dilate the rectum, so as to open and form a free communication with the colon, and thereby not only give exit but circulation to the matter so confined." This is effected by "the introduction of a large sized gum elastic tube, through the anus into the sigmoid flexure of the colon, and after giving exit to such flatus and fluid feces as may happen to escape, adapting to

it a proper syringe, and throwing up such purgative fluids as circumstances may make it necessary to select." For the directions necessary to be observed in effecting this, and the signs indicating when the instrument has reached its destination, we must refer our readers to the work itself. A timid operator might entertain a fear of rupturing the intestine, in using the necessary degree of force. Dr. O'Beirne assures us there is no cause for such fear. We have dwelt at some length on the new physiology put forward by our author, because on the physiological foundation the whole afterpart rests. We have been very brief on the pathological portion, and have been obliged to omit many things that our readers might desire to become acquainted with; we now come to the details of treatment, and these are given in the best form, namely, in the relation of individual cases, with appended remarks. A single case of any disease, graphically described, presents to the mind of the reader a better idea of the disease than the most learned and laboured abstract description. We shall select a few of the cases. The first case in which the tube was used with good effect, is one of retrocedent gout, in which the curious and unexampled fact occurred, of the metastasis of gout to the intestines, producing from their mucous surface a flow of serous and limpid fluid to the amount of several pints. The second case is one of spinal irritation, in which, for nearly six months, the only food taken was a little milk and lime water, always vomited soon after being swallowed, and accompanied with obstinate constipation for the same duration of time. The spasmodic affection of the rectum to which the constipation was owing, disappeared under local treatment of the spine, and the use of the long injecting tube. The next case, furnished to the author by Dr. M'Dowel, Surgeon to the Richmond Hospital, places Dr. O'Beirne's views in a very high position.

"*Mania*.—I am indebted to my colleague, Doctor Ephraim M'Dowel, one of the surgeons of the House of Industry, and Richmond Surgical Hospital, for the following case.

"On the 21st of February, 1830, Miss D., aged 17, and in whom the catamenia had become suppressed for some time previously, attempted to cut her throat. The wound, although extending from ear to ear, was fortunately very superficial, and the hæmorrhage trifling. Being discovered, immediately after, in this situation, one of my pupils, who happened to reside in the neighbourhood, was instantly sent for, and he dressed the wound. Shortly after I was called to attend her, and finding her in a very sullen and melancholy mood, and that her bowels had not been freed for some time before she committed the act, ordered her infusion of senna, with tartarized antimony, which operated freely. During several days she continued in the

same sullen state, but at length became violent, and in short, so maniacal as to require restraint. Her bowels now becoming confined, large and repeated doses of calomel and cathartic extract, in the form of pill, were ordered and regularly administered, but without producing the least effect. Draughts of castor oil and oil of turpentine, of infusion of senna, electuary of scammony, tincture of jalap, and sulphate of magnesia, were then exhibited, but also ineffectually. Croton oil, in doses of three drops at a time, and frequently repeated, was next had recourse to, but it likewise failed to move the bowels. Enemata, consisting of infusion of senna, electuary of scammony, tincture of jalap, oil of olives, oil of turpentine, and sulphate of soda, administered by a syringe, to which the common ivory pipe was attached, proved equally unsuccessful; and the same may be said of persevering friction of the abdomen, with various kinds of stimulating liniments. At length, when the constipation had existed thus obstinately for seven days; when the tongue had become furred, brown, and dry, the pulse feeble and intermitting, and the delirium of a low muttering nature, the tube of a stomach pump was passed up the rectum, and with some difficulty. As soon as it had reached the height of seven or eight inches, a loud burst of flatus, followed by a great and continued flow of liquid feces, took place from the tube, and produced immediate, decided, and general relief. As the bowels continued to act for nearly two hours after the tube was removed, no injection was thrown up, or medicine of any kind ordered. All this occurred about half-past eleven o'clock at night. She slept soundly that night. The following morning I found her pulse natural, her tongue moist and nearly clean, her strength greatly recruited, and she had scarcely any remains of delirium. From this period the improvement in her mental and bodily health was progressive and rapid, and she recovered perfectly.

"I have frequently employed the tube, in the same way, to remove obstinate constipation, and uniformly with the most decided success."—pp. 87 to 89.

Under the head of Abdominal Tumours, page 95, the first case is an exemplification of a fact not to be forgotten, that the sigmoid flexure of the colon may, in consequence of sedentary occupation or long continued constipation, become the seat of lodgments of feces, similar to those met with in the cæcum, and by the swelling produced, give rise to the supposition of the tumour being a new growth.

We now come to a most important part of the work before us; the application of the principles and practice put forth, to the relief of cases of strangulated hernia. The relief of even a single case from the terrors and danger of an operation, is an object of the first consideration. We shall first give our author's views of the mechanism of strangulated hernia, and the indication to be followed in attempting its reduction.

“Strangulated Hernia.—It is manifest that the openings or rings through which portions of the intestinal canal escape from the abdomen and form herniæ, cannot, from their tendinous nature, as well as from their peculiar formation, be considered as acting otherwise than as passive agents in causing strangulation, and that, with the single exception of cases in which the neck of the hernial sac becomes considerably thickened and contracted, strangulation is always caused by the prolapsed portion of intestine becoming so distended, generally by the gaseous and very rarely by the fluid or solid contents, as to be pressed forcibly against the margins of the opening or ring, and to be no longer capable of repassing through it, and returning into its natural situation in the cavity of the abdomen. These points have been ably and successfully urged by Wilmer, Geoghegan, and others, and seem to be now generally admitted and acted upon. But these and all other writers have failed to notice or take advantage of another self-evident and very important fact respecting the condition of an intestine so situated; namely, the fact that strangulation can never be so tight or excessive as to prevent a more or less free passage of air through the hernial tumour. The very circumstance of strangulation being an effect of pressure exerted from within, not to speak of the exceedingly subtle and permeating quality of air, and the force with which it acts, would even shew that in such cases the gaseous contents must circulate with considerable freedom.

“Such being, with the exception already mentioned, the actual state of the facts respecting the manner in which an intestine becomes strangulated, and the condition in which it is afterwards placed, it must be obvious that the volume of an intestine so situated, must be diminished to a certain extent, before reduction can be effected.”—pp. 107, 108.

Dr. O’Beirne next runs rapidly over the various means hitherto employed for reducing the bulk of the protruded intestine, and then proposes his own mode.

“To introduce a gum elastic tube into the colon, and to leave it there until the large intestines, and eventually the hernial tumour are emptied of their gaseous contents. If the bowels happened to be well freed, or to contain but a small portion of solid feces, at the time the strangulation took place, success might reasonably be expected from this mode of proceeding; and if they happened to be loaded with solid matter at that time, it would only be necessary to introduce the tube more frequently, and at intervals of a few minutes between each introduction, first, to empty the sigmoid flexure, secondly, the cæcum, thirdly, the hernial tumour, in order to effect the object in view. It is no argument against the proposed plan, that Mr. Hey, and other experienced practitioners, have declared that evacuations from the bowels rarely effect the reduction of a strangulated hernia; for it should be remembered that, on such occasions, the rectum contracts the moment it has discharged its contents, and permits but a comparatively small and insufficient quantity of wind to escape, whereas, ac-

cording to the mode which I propose, the flatus is offered every facility of passing off in a continued stream, and for as long a time as may be found necessary. In one case the contents of the flexure and a portion of the gas contained between it and the cæcum, are evacuated, while the latter is left distended ; so that if the bowels should happen to contain much fecal matter, such is the peculiarity of the process by which the cæcum and sigmoid flexure are filled and emptied, and such the rapidity with which the rectum contracts after discharging itself, that repeated stools may take place, without producing any favourable impression on the strangulated portion of the intestine. In the other case, no matter how distended it may be, we have it fully in our power to rapidly empty the whole of the intestinal canal, and thus insure the reduction of its strangulated portion.”—pp. 113, 114.

Several cases are given in illustration of the good effects of the line of treatment recommended. We shall make room for two ; one is contained in a letter to the author from Mr. Hayden, Surgeon to the Anglesey Lying-in Institution.

“The case of strangulated hernia occurred two years ago, and was that of an old man who lived in a wretched and filthy hovel in Hanover-square, off Hanover-lane. The hernial tumour was situated a little below the umbilicus, and had resisted all attempts at the taxis. The case was seen by yourself and Mr. Robert Adams, in consequence of considering it one that would require operation. In consultation, you proposed to try the effects of enemata given through the usual gum elastic tube. His bowels had not been moved for several days. I introduced the instrument myself with great ease. On arriving at the sigmoid flexure of the colon, a considerable quantity of flatus and fluid feces flowed through the tube ; the patient expressed himself as feeling greatly relieved, and on examining the situation of the hernia, the tumour had completely disappeared. I next threw up (without withdrawing the tube) a strong cathartic enema, which, in a few minutes, produced a copious discharge of solid and liquid feces. These are the prominent features of this case.”—pp. 217, 218.

The second is a most satisfactory case in point.

“On Saturday, the 15th of December, 1832, after prescribing for a gentleman confined in the Four-Court Marshalsea, I was requested to visit Mr. John F———, who was said to be very ill in one of the rooms of the prison. On seeing him, he appeared to be under thirty years of age, and very robust ; his countenance was pale and expressive of acute pain, and his pulse quick, hard, and rather small ; he complained of severe pain in the abdomen, stated that his bowels had not been freed for several days, and pointed to the hearth, where, he said, he had just vomited up the contents of his stomach. After his friends had retired at his request, he informed me that he had been afflicted with rupture for several years ; that he had never worn a truss ; and that although the bowel had frequently descended, it was

always easily returned until early that morning, when he failed in reducing it as usual. On examination, I found that the abdomen was tense and painful on pressure, and that he had a moderate sized inguinal hernia on the right side. The hernial tumour was very tense, particularly at the ring, slightly suffused with redness, and seemed to be altogether formed by prolapsed intestine distended by air, and, as far as the touch enabled me to judge, containing a small quantity of feces. Immediately after ascertaining the nature of the case, the patient was placed on a sofa, in the usual position, and attempts were made at the taxis. These attempts were made four or five times, at distinct intervals, and with as much force as could be safely employed, or as he could bear, for he complained of the severe pain which they gave him; but I found it impracticable to reduce the hernial tumour, and improper to persevere longer in efforts for that purpose. Finding that I had failed, he informed me that he had sent for a neighbouring apothecary to take blood freely from his arm, and apply cold lotions to the scrotum, as these means had saved him from an operation when he was first ruptured, and he earnestly entreated me to adopt the same practice. But I succeeded in inducing him to wait until I could procure a tube and try its effects. Accordingly a messenger was despatched to my house for the necessary apparatus. In the mean time, the apothecary, Mr. Carroll, of Meath-street, arrived, and was requested to go for a purgative injection, composed of a pint of the common enema, an ounce of oil of turpentine, and the same quantity of sulphate magnesia. As soon as this and the apparatus were procured, the patient was turned on his left side, with his knees drawn up, and the tube, which happened to be a stomach tube, after being oiled, was passed up the rectum, without much difficulty, to the height of about ten inches. No flatus could be heard escaping, but Mr. Carroll, who assisted me, on placing his hand close to the lower extremity of the instrument, said that he distinctly felt a current of air passing from it to his hand; and, on looking at the hernial tumour, we both agreed that it was sensibly diminished in size: the patient also said that he was certain of the fact, and assured us that he felt himself considerably relieved. This appearing to be a favourable moment for attempts at the taxis, they were renewed, but proved ineffectual. Considering the failure to arise from the eye or opening in the side of the tube having become blocked up with solid feces, I removed the instrument, and found that I had judged correctly. The tube was now well washed and syringed out with cold water, and after being dried and then oiled, was again introduced, and passed up to a greater height. Immediately a still greater reduction took place in the hernial tumour, and, on making another attempt at the taxis, the tube still remaining within the bowels, it succeeded, and the strangulated intestine was completely reduced. The patient immediately felt himself freed from all pain and distress, but as the sigmoid flexure was evidently loaded with solid feces, it was considered necessary to throw up the enema

which had been prepared. His bowels acted freely soon after, and, on visiting him the following day, I found him perfectly well, but lying in bed, as he was directed, until a proper truss could be applied."—pp. 212—222.

Under the head of Colic and Enteritis, several instances are detailed, in which the tube, used according to the author's direction, effected instantaneous abatement of the symptoms. Under the same head there are some useful observations on the impropriety of giving strong purgatives by mouth in Enteritis; indeed no practitioner who knows his profession, would, in the present day, attempt to overcome the constipation which accompanies this disease by purgatives, until he had first lessened inflammatory action. Under the same head, however, Dr. O'Beirne betrays the tendency which every man exhibits who has made a discovery, viz., to ride his hobby to death. Not content with giving sufficient instances of the benefits resulting from the application of his discoveries, he would attempt to make us consider the inflammation in enteritis as merely a secondary effect, and would endeavour to persuade us, because we cannot have *ocular evidence* of the fact being so, that we are not to consider tenderness on pressure, buffed blood, &c., as symptoms of enteritis.

After a few cases under the head of Puerperal Fever comes a chapter on Dysentery. Dr. O'Beirne denies that the pathology of this disease is originally inflammation of the mucous membrane of the colon, and asserts, on the contrary, that the whole mucous membrane of the digestive canal is equally affected in the commencement, and that the appearances of inflammation, ulceration, &c., which are found in the colon, result from the morbid secretions of the intestinal canal being arrested there, and by their presence producing ulceration, while the small intestines relieving themselves of their morbid secretion by pouring them into the colon, thus escape the bad effects that might otherwise result. We think that on this part of pathology Dr. O'Beirne has failed, and that his mode of reasoning is weak. For instance the following is one of his arguments. He speaks thus: "If one of the first morbid actions which produce the disease consists, as there is every reason to believe, in the determination of blood from the surface of the body to the abdominal organs, particularly the liver, stomach, and intestines, how can the seat of the disease be said to be confined to the mucous surface of the large intestines?" If such a mode of argument be admitted we would be able by means of it to prove that inflammation of the liver has its seat in the intestines, for we might say, If one of the first causes of the disease (hepatitis) be the determination of blood to the abdominal organs, &c., how can the

seat of the disease be said to be confined to the liver? The question as to the pathology of dysentery is one of facts not of arguments, and Dr. O'Beirne has not brought sufficient facts to support his views. Here, as in the instance before alluded to, Dr. O'Beirne is weakening himself by attempting to prove too much; although his pathology of the disease may be defective, it does not follow that his practice is erroneous. He brings facts quite sufficient to prove that the tube may be passed up into the colon in dysentery, not only without danger, but with the most decided and immediate good effects, and he might well rest satisfied with this. We shall give a case.

"CASE 2. Anne Wilson, aged 30, nurse of ward No. 25, House of Industry, complains this day, January 8th, 1833, of loss of appetite, nausea, retching, griping pains in the abdomen, particularly around the umbilicus, and of frequent desire to go to stool, the calls to stool being followed by mucus tinged with blood, and occasionally with a few small scybala. Tongue white, pulse 120, and small; left iliac region tender under pressure. She states that she was seized, three days ago, with shivering, followed by tormina, tenesmus, and other symptoms which have been enumerated, and that her bowels had been constipated for nine days previous to the attack. The tube was immediately passed up the rectum, but with much less pain and difficulty than in the former case, and as soon as it reached the necessary height, an offensive smell indicated the escape of flatus from the intestines. An injection, consisting of one quart of warm water, and two ounces of castor oil, was thrown up, but returned in a few minutes without any admixture of fecal matter. Considering that this circumstance indicated an accumulation of fecal matter in the cæcum, and not in the sigmoid flexure of the colon, and expecting that the evacuation of air from the whole of the colon, would cause the accumulated matter to be transferred from the former to the latter situation, I waited for a quarter of an hour, and then re-introduced the tube, which gave her considerable pain, and threw up a similar injection, but containing a double quantity (two quarts) of warm water, and half an ounce of sulphate of soda. In a few minutes the bowels were freed, and discharged a considerable quantity of flatus, mucus, and hardened feces; all griping pain of the abdomen disappeared, and the pulse became much fuller, and fell to 96.

"9th, 2 o'clock, P. M. Has had two copious natural stools, but neither tormina nor tenesmus since yesterday. The tongue somewhat white, and the pulse 80, soft and full; but she complains of an uneasy sensation about the umbilicus. Ordered to have the tube introduced again at eight o'clock in the evening, and a quart of warm water, two ounces of castor oil, and the same quantity of olive oil, to be injected through it into the colon.

"10th, 9 o'clock, A. M. Has slept well, and had several natural

stools, during the night ; and she is now quite recovered."—pp. 284, 285.

In conjunction with the use of the tube, Dr. O'Beirne is a warm advocate for the employment of tobacco in dysentery, colic, &c., and at page 170 gives the directions for using this plant. Under the article Tympanites are given some cases, with dissections extracted from the "*Dictionnaire des Sciences Medicales*," which it is, indeed, impossible to read without being convinced, that had the view now, for the first time, offered, of the function of the rectum been understood, the result of some of these cases would not have been fatal.

The last part of the book is taken up, and we think very unnecessarily, by a discussion on the author's claim to "the origination of the plan of mechanically dilating the rectum, and employing the gum elastic tube for this purpose, &c." Even if it were established that the tube had been used by others previously to Dr. O'Beirne, this would not at all affect his claims. He has been the first to establish its application upon scientific principles. Harvey's discovery first revealed to us the scientific application of blood-letting, and his merit is not the less because venesection was employed before the discovery of the circulation. We hope we have succeeded in clearly explaining to our readers the novel physiological doctrines of the work before us. Those doctrines upon which the superstructure of pathology and treatment is raised are perfectly original, and our author more fortunate or more talented than most discoverers, has at once established them beyond the reach of cavil or doubt. We have differed on some points from Dr. O'Beirne ; we should, however, be wanting in justice to him were we not to say that we consider his work as one containing the most valuable and practically useful information on the treatment of diseases of the abdominal organs. We confess that we sat down to peruse his work as sceptics ; we have risen from its perusal converts to his opinions, and we conscientiously think no practitioner henceforth would be justified in undertaking the treatment of a case of serious abdominal disease, without being thoroughly acquainted with our author's principles and practice. In our next number we shall bring before our readers the second part of Dr. O'Beirne's work, his critique on Sir C. Bell's *Views of the Nerves of the Face*.

SCIENTIFIC INTELLIGENCE.

CHEMICAL AND PHYSICAL SCIENCE.

Analysis of Variolous Crusts.—The experiments we now publish were undertaken towards the close of the year 1830, on some variolous crusts sent us from Marseilles, by Dr. Roux, who has paid particular attention to this affection. These variolous crusts were enclosed in three packets sealed and numbered. The first contained *variolous crusts of the ordinary character*; the second *variolous crusts with petechiæ*, and the third *crusts of a particular nature*. In this last packet there were but three crusts of from 5 to 6 millimetres in diameter, weighing in all 0,110, ou deux grains 1.5. This very small quantity prevented us from making a complete analysis of them; we shall therefore pass over in silence the results obtained, so much the more, as we know not under what circumstances these crusts were collected.

1. An equal quantity of Nos. 1 and 2 were brought in contact at the temperature of 68° , with thirty times its weight of distilled water. At the end of twelve hours' maceration, it was filtered in order to separate the insoluble matter, which was washed with a quantity of cold water equal to what had been employed at first. The lixivium added to the filtered liquor were evaporated at a moderate heat in a porcelain capsule previously dried, so that one might know at once by the increase of weight in the vessel, the proportion of the matter dissolved by the water. It was observed during the evaporation of the liquor, that it became turbid, and threw down flakes of albumen, which were subsequently separated by treating the watery extract with alcohol at 30° .

The portion of this extract dissolved by the alcohol, on being carefully examined, exhibited the presence of chloride of sodium, chloride of potassium, and an animal substance of a yellow brownish colour, not crystallizable, having the odour of boiled meat, similar, in most of its properties, to osmazome.

2. The crusts after being thus treated with water, were brought in contact with boiling alcohol, which extracted from them a certain quantity of white, semi-solid fatty matter, at the ordinary temperature; lastly, after the action of these two vehicles, the insoluble matter of these crusts presented, together with some distinct portions of epidermis, a semi-transparent substance, of a horny appearance, soluble in caustic potash, with disengagement of ammonia, and participating in the properties of coagulated albumen.

3. The fixed saline matters were ascertained and estimated by incinerating, in a platina crucible, an equal quantity of these two species of crusts, and treating the ashes first with water, in order to extract the soluble alkaline salts, and then with water acidulated with hydrochloric acid, in order to dissolve the calcareous salts.

Though we have not been able to vary our experiments and carry them further, we are warranted in concluding, that these different morbid products, setting aside the proportions of the principles discovered, present the greatest analogy among themselves, as the following summary proves.

No. 1.—*Crusts without Petechiæ.*

Insoluble residue consisting of shreds of epidermis and of coagulated albumen	70
Albumen soluble in cold water	15
Fatty matter	1,3
Animal matter similar to osmazome	11,2
Chloride of sodium, chloride of potassium, subcarbonate of soda, phosphate of soda, phosphate of lime	2,5
	<hr/> 100.0

No. 2.—*Crusts with Petechiæ.*

Insoluble residue consisting of shreds of epidermis and of coagulated albumen	63,2
Albumen soluble in cold water	14,3
Fatty matter	2,3
Substance similar to osmazome	18,1
Alcaline and calcareous salts	2,1
	<hr/> 100.0

The presence of osmazome in these crusts goes to confirm the observation which was made ten years since, by our colleague, M. Chevalier, with respect to the existence of this immediate principle in purulent matter and other morbid productions.—*Journal de Chem. Med.* Dec.

Cause of Crystallization—M. Gaudin has addressed an article on certain properties of atoms, the object of which is to point some particular cases of an extensive series of experiments regarding the immediate causes of crystallization. According to him, those bodies cleaving in a cube have for their primitive molecule a point or a right line; either an atom (as in metals); two dissimilar atoms (as in sulphuret of lead); three atoms, one of one kind, and two of another, (chloride of sodium): again, those molecules having for their form a double simple pyramid, or a prism of a number of sides, which is a multiple of four, may be assimilated to points when under different

circumstances they form in groups around an accidental nucleus, and in this way they sometimes crystallize in cubes. Those bodies which cleave in a *right prism*, with any base whatever, have for their primitive molecule, either the polygon of the base, or a double simple pyramid, or a prism of a number of sides equal or double. When the primitive prism is not cleavable perpendicularly at the axis, it is because the molecules are polarized, that is retained together either by an electric force, or by the force of affinity. The tetraedron, octaedron, and rhomboedron have respectively for their primitive molecule, the first a square, or a rhomb; the second a double tetraedral, or octaedral pyramid, simple or prised, (*prismée*), and the third a double hexaedral pyramid of the same nature. Finally, the absence of cleavage in these solids is owing to the want of continuity of the molecules. But in the tetraedron the want of continuity depends solely on this state of polarization, whilst in the octaedron and rhomboedron it results from the aggregation of the molecules.—*Ibid.*

Preservation of Iron from Rust.—M. Thenard has reported favourably on the observations of M. Payen regarding the property which alkaline solutions possess, of preventing the oxidizement of iron and steel. “The fact,” says M. Payen, “is beyond doubt, and a circumstance worthy of remark is, that a very small quantity of alkali suffices to produce this result. Let bars of iron or of steel be plunged into common water, and their oxidizement will not be at all retarded: $\frac{1}{4000}$ of caustic potash will not preserve them from oxidation; but $\frac{1}{2000}$ of this alkali, or of lime water, mixed with its own weight of water, and a fortiori, pure lime water will preserve them from it. Alkaline carbonates and borax, but in much larger proportions, produce the same effect. If then water holding in solution $\frac{1}{2000}$ of an alkali were in contact with the air, in proportion as the alkali would attract carbon, the iron or steel immersed in it might be oxidized. Similar phenomena might probably occur with other metals; it would be curious to submit manganese, zinc, and arsenic to the same process. Probably, water to which there should be added not more than $\frac{1}{2000}$ of an alkali, but the same proportion of acid, would produce a quite contrary effect, and the iron would be rapidly oxidized. Is not this one of the reasons why iron filings, sprinkled with water containing a little vinegar, is so easily rusted?”

“Be this as it may,” adds M. Thenard, “it is not improbable, that the observation of M. Payen may admit of useful applications. This chemist has already pointed out some; but there are others more or less important, which may now be particularized. Might we not then preserve iron, which would be subjected to no friction, and which, moreover, should be sheltered from rain, by moistening it with an alkaline solution? Why not try to preserve cannons in the same way? and when they are mounted on wood, what is there to prevent one from placing on the cannon after use a sponge soaked in an alkaline solution? The reporter assures us, that an iron bar immersed in an alkaline solution, and then left exposed to the air in a

moist place, had not, at the end of twelve hours, lost any of its brightness. If the iron were not protected from rain, it might perhaps be sufficient to cover it with an alkaline coat, and then over the coat a layer of varnish. The iron pillars used in metal bridges might probably be preserved from rust in a similar way. The same may be said, no doubt, with respect to iron employed in buildings, and intended to penetrate into the interior of the walls."

M. Thenard relates the fact of two cannons which were deposited in a cellar for better than a week; of which the one, which had been coated with an alkaline solution, to which a small quantity of gum had been added, had lost nothing of its brightness, whilst the other, which at the commencement of the experiment was equally bright, was soon covered over with rust.—*Ibid.*

Biographic Memoir of Chaptal.—One of the men whose name is associated with so many varieties of useful knowledge, Baron Cuvier, has said, that the essential object of the sciences is to conduct the human mind to the knowledge of truth, by diffusing correct ideas even amidst the humblest walks of society, and by withdrawing men from the influence of their prejudices and passions, so as to make experience and reason the arbiters and supreme guides of public opinion. Such is the end which Chaptal invariably proposed to himself. Thus, the best mode of eulogizing him will be to refer to his own works, in order to shew what he has accomplished for science, as well by his own individual exertions, as by affording a paternal support to every thing that could contribute to advance it.

Jean Antoine Chaptal, count of Chanteloup, was born in the year 1756, at Nozaret, in the department of Lozere. After finishing his studies in the college of Rhodéz, he went to Montpellier to study medicine under the care of his uncle who was professor in that school. Having obtained his doctor's degree he repaired to Paris, not in order to perfect himself in his medical studies, for the university of Montpellier was at that time far more celebrated as a school of medicine than that of Paris, but for the purpose of cultivating a science of which he had but saluted the outlines. Chemistry possessed for young Chaptal that powerful attraction by which it is so well characterized, inasmuch as instead of losing itself in the vagueness of conjecture, it not only furnishes the explanation of natural phenomena, but also, itself produces substances and new phenomena, and frequently makes the happiest application of them to medicine and the arts, at the same time that these its creations oftentimes become the source of new phenomena. It was at the school of the celebrated men, who prepared the grand work of pneumatic chemistry, and who so signally improved chemical nomenclature, that Chaptal acquired that variety and extent of knowledge which was destined to enrich the south of France. On his return to Montpellier the states of Languedoc established a chair of chemistry, which, though at first promised to Pourret, was, despite of the influence of Cardinal Brienne, conferred on Chaptal. Nor was he long in justi-

fying this honourable choice. Endowed with all the moral and physical qualities which were necessary for such a situation, he inspired into his pupils the same ardour and enthusiasm for the science, which he felt himself. He exerted himself particularly in developing those truths which constitute the body of the science, and in separating them from those ill-conceived experiments and extravagant hypotheses, which, like the brilliant error of Stahl regarding phlogiston, are destined to sink into oblivion. M. Chaptal in his lectures compared the science of chemistry to an immense pillar, raised in the midst of ages, on the indestructible basis of well observed facts amidst the scattered ruins of vague hypotheses; nor was he, however, forgetful of the merits of the ancient chemists, to whom he was ever ready to pay the just meed of their laborious exertions. The lectures of Chaptal became so interesting as to be attended even by the professors of the school of medicine, whose colleague he soon became. With the most scrupulous attention was the young professor listened to, when he disclosed to view the history of the science, and dwelt on those ages of barbarism, when the chemist scarcely ventured to acknowledge the occupation which in secret constituted his sole delight. At that time the name of chemist was almost an opprobrium, and the prejudice which existed against the alchemists, retarded for many centuries the revival of those arts to which chemistry serves as the base. M. Chaptal soon perceived that chemistry was but an object of idle curiosity, unless it presented applications useful to the wants or the preservation of life. For this reason his lectures invariably contained applications to the arts, to agriculture, to medicine, &c. When addressing the manufacturer, he pointed out to him the connexions subsisting between the arts, &c., which illustrate their principles, reform their abuses, simplify their means, and accelerate their progress. In addressing the physician, he pointed out to him that the abuses committed in the 18th century, by the irrational application of chemistry to medicine, caused men to overlook the natural and intimate connexions of this science with the healing art. In order to direct with good effect its application to the human body, it is necessary to combine an extensive and accurate knowledge of the animal economy, with a profound knowledge of chemistry; the results of the laboratory must be made subordinate to the observations of physiology, the one should be illustrated by the other, and nothing acknowledged as a truth, but that which is strictly conformable to the principles of each. When directing his attention to pharmacy, he pointed out the analogy which unites it to chemistry so closely, that they were for a long time considered as one and the same science. True it is, that the latter has been for a long time cultivated only by apothecaries, and from their laboratory have come forth most of the discoveries of the 19th century. The agriculturist found in his lectures a summary of all the aids which chemistry can afford to agriculture. To render them still more profitable, he published his *Course of Chemistry*, in 1 vol. 8vo., which in 1790 appeared in 3 vols. 8vo., under the title of *Elements of Chemistry*, and went through four editions. It pre-

ceded by two years Lavoisier's *Elementary Treatise on Chemistry*, and, consequently, was the first book which spoke the language of the new chemical nomenclature. It was soon translated into several languages, and spread over Europe. In addition to its many other recommendations, it combined depth of reasoning with the fascinating embellishments of a most brilliant imagination; so that the sacred fire of genius seemed in a manner to enliven all its pages. At length the fame of Chaptal became so great, that Washington endeavoured more than once to prevail on him to illumine by the torch of chemistry the rising arts of the new world. Several other very flattering offers were made to him about the same period, but to them all Chaptal was deaf; he looked to nothing but the prosperity and glory of his country. At the time when Paris became the very centre of danger during the revolution, he betook himself to that capital, and being employed by the government, he rendered to France the most essential services, by the improvements he introduced in the manufacture of gunpowder. Some time after, when success began to crown the arms of France, Chaptal was sent to Montpellier in order re-organize there that school of medicine, which was all his life the object of his affections, and which, in its turn, regarded him as its guardian angel. He was afterwards appointed professor of chemistry to the Polytechnic school, with Berthollet, Fourcroy, Guyton de Morveau, and Vauquelin.

After Bonaparte had overturned the Directory, and on its ruins established his own power, he thought that there was no better mode of securing it than by encircling France with the halo of glory and of science. He was therefore anxious to attach to his interests the most distinguished scientific characters, with whom he enriched the senate and the council of state, and in testimony of the signal deserts of Chaptal, appointed him to direct the national instruction, an office for which no person could be better qualified. The multiplicity of his occupations in this office interfered not with his scientific pursuits. He published at this period his work on *The Perfecting of the Chemical Arts in France*. His zeal and scientific exertions procured for him soon after a more distinguished elevation, by his being appointed minister of the interior, an elevation which may well be considered a triumph, as well as a blessing, for science. The opportunities afforded him in this office he devoted almost exclusively to the improvement of the mechanical arts, as well as those of chemistry and agriculture. It was at this period also, he laid the foundation of his work entitled *Chemistry applied to the Arts*. After rendering to France the most signal services, by the improvements he introduced in her manufactures, as well as by the encouragement of her national industry, by the establishment of public schools where the several mechanical arts were taught; having also effected the most important amelioration in the state of the principal hospitals of Paris, with regard to the internal arrangements of these asylums of suffering humanity, Chaptal resigned his office. Napoleon, however, not unmindful of the splendid services of his minister, elevated him

to the senate, and honoured him with the grand cross of the legion of honour. After the emperor's fall Chaptal continued faithful to him. Entering into private life he resumed his favourite studies, and devoted himself particularly to agricultural pursuits. In 1819, however, he was called to the peerage, and during the fourteen years he enjoyed it, he shewed himself the eloquent defender of liberty, as also the warm supporter of commerce and of agriculture; nor was he, however, amidst such weighty occupations, inattentive to his favourite science. Independently of 24 memoirs which he published on the chemical arts, we are indebted to him for several others, of which the principal are *Elements of Chemistry*, 3 vols. in 8vo., 4 editions. *Essay on Bleaching*, 1 vol. in 8vo., 1801. *Chemistry applied to the Arts*, 4 vols. in 8vo. *Chemistry applied to Agriculture*, 2 vols. in 8vo. The last work was the summary of the labours of his life regarding the progress of agriculture, which he always considered as the purest source of national prosperity, and one in which civilized man was deeply interested, and which he accordingly was ever desirous of raising to the rank of a science. Chaptal was peer of France, minister of the interior, minister of state, and director of commerce, of the arts and manufactures; director of national instruction, grand officer of the legion of honour, knight of the order of St. Michael, professor of chemistry in the Polytechnic school, as well as in the school of medicine of Montpellier; member of the Royal Academy of the sciences, of the Royal Society of London, &c. We cannot conclude the present memoir better than by extracting the following passage from the discourse of Dupin pronounced on the subject of it. After having acquired, says Dupin, every species of glory, which the philosopher and citizen can ambition, after having devoted sixty years out of seventy-six to promote the interest and honour of his country, Chaptal saw himself deprived of his fortune, the entire of which he gave up to relieve the embarrassments of the heir of his name: nor did the circumstance elicit from him a complaint. After experiencing this reverse, weighed down with anxiety for the well-being of his posterity, the only consideration which could cause him sorrow, Chaptal died on the 30th of July, leaving the brightest halo around his memory in those brilliant services, of which France shall cherish the recollection to the remotest ages.—*Ibid.*

Poisoning by Arseniuretted Hydrogen.—M. Ollivier of Angers makes an observation on a case recorded by M. Fumey of Coligny, relative to a disease which manifested itself in several individuals of the same family, who presented symptoms similar to those of cholera. The author considers that this affection was occasioned by a disengagement of arseniuretted hydrogen gas in a privy into which some arsenic had been thrown. M. Ollivier thinks that this privy having no chimney nor a free current of air, the arseniuretted gas could not ascend by reason of its specific gravity, and also that the gas could not be formed under these circumstances. He regards the opinion

of the author as void of foundation. M. Landibert thinks, with M. Ollivier, that arseniuretted hydrogen could not have been formed in this case; but admitting its developement, he thinks that it might ascend stratum by stratum, notwithstanding its great specific gravity.—*Archives Generales de Medicine*, Dec. 1832.

Analysis of the Water of Rio Vinagre, by M. Boussingault.—A phenomenon which deserves to engage the attention of those travellers who visit Popayan is the Rio de Pasambio, the waters of which are acid, and for this reason the inhabitants of Cordilliere call it Rio Vinagre, the river of vinegar. The Rio Vinagre rises near the volcano of Puracé, at an elevation of about 4300 metres. As far as the village of Puracé the stream has a subterraneous course, and it is only at Chorrera de San Antonio that the Rio Vinagre can be approached with ease. Chorrera de San Antonio presents an admirable cascade, falling from a height of more than 300 feet, into the midst of an extensive amphitheatre. Two or three miles lower down than Chorrera, the Pasambio, after receiving the Anambio, empties itself into the Cauca. From the village of Puracé we arrive without much difficulty below Chorrera; but it is painful to remain, by reason of a continual raining of acidulated water, which occasions in the eyes an intolerable itching. A little below the cascade I found that the Rio Pasambio was 72 feet in breadth, with a mean depth of 4 inches (pouces); the velocity of its course was 3 feet per second. The water of the Rio Vinagre is perfectly limpid; its specific gravity is 1.0015; it possesses a well-marked acid taste, mixed with an astringency which denotes a salt of alumina; it reddens tincture of turnesol, even after it has undergone continued boiling; when placed on zinc filings it occasions a sensible disengagement of hydrogen. Reagents indicate in this water sulphuric and hydrochloric acid, lime, alumina, and traces of iron and magnesia. I shall here state the results I obtained in an analysis made at Puracé in April, 1831. 422 grammes of water of the Rio Vinagre gave with nitrate of silver 2.01 grs. of a chloride, equivalent to 0.384 gr. of hydrochloric acid. 422 grammes of water, treated with chloride of barium, produced 1.35 grs. of sulphate of baryta, containing 0.464 gr. of sulphuric acid. 422 grammes of the water of Vinagre were reduced by evaporation; caustic ammonia poured into this water gave rise to a precipitate of alumina which weighed 0.17 gr.; this alumina contained iron and magnesia. In the liquor deprived of alumina, the oxalate of ammonia precipitated the lime; the oxalate of lime, converted into a carbonate, weighed 0.10 gr., containing 0.056 gr. of lime. The liquid then containing neither lime nor alumina was evaporated, and after driving off the ammoniacal salts, there remained a residue of alkaline salts with a base of soda. These salts were changed into a sulphate; the sulphate of soda at first weighed 0.13 gr.; but on dissolving in water it yielded 0.01 gr. of silica; its weight was thereby reduced to 0.12 gr., amounting to 0.05 gr. of soda. According to this analysis the water of Rio Vinagre contains

Sulphuric acid	.	.	.	0.00110
Hydrochloric acid	.	.	.	0.00091
Alumina	.	.	.	0.00040
Lime	.	.	.	0.00013
Soda	.	.	.	0.00012
Silica	.	.	.	0.00023
Oxide of iron, magnesia,	.	.	.	some traces.

If we admit that the alumina and lime are combined with the sulphuric acid, the composition of the Rio Vinagre may be expressed as follows :

Sulphate of alumina	.	.	0.00131
Sulphate of lime	.	.	0.00031
Chloride of sodium	.	.	0.00022
Silica	.	.	0.00023
Hydrochloric acid	.	.	0.00081

This is, however, but a supposition : it is even probable that it is rather to the sulphuric acid than to the hydrochloric acid that the water of the Pasambio owes its acidity ; for I could not discover any free hydrochloric acid in the products of the volcano of Puracé. In the crater of the volcano of Pasto I discovered a considerable quantity of an acid sulphate of alumina, which communicates to water an acid and astringent taste ; the analysis of this sulphate, which I intend to set about forthwith, will no doubt throw some light on the composition of the salts contained in the water of the Rio Vinagre, and on the nature of the free acid found in it.—*Annales de Chimie, Sep. 1832.*

Experiments on Platina, by Mr. Phillips.—The third volume of the Quarterly Journal of Science contains a paper, by Mr. J. T. Cooper, *On some Combinations of Platinum.* In this communication the author states, that when a neutral solution of tartrate of soda is heated with one of muriate of platina, a blackish powder is precipitated : this substance after being washed, was dried on a sand-bath at 300°, in order to free it from uncombined water : it lost afterwards 2.8 per cent. by exposure to a red heat ; and as nothing could be procured from the black powder but platina and water, Mr. Cooper considers it to be a hydrate of the metal, composed of $44.328 = 2$ atoms of platina $+ 1.125 = 1$ atom of water : these proportions agree tolerably well with the results of the experiment.

It is singular that Mr. Cooper does not particularly advert to the interesting fact which he announces ; for this is, I believe, the first instance on record of the combination of water with a metal, not previously converted into an oxide ; and it is almost as remarkable, that of the numerous authors whom I have consulted on the subject, no one mentions this compound.

Although, with some particular views, I have repeatedly formed this black powder, it is only lately that I have investigated its properties. Having dissolved some platina, and precipitated it in the manner described, I duly washed the powder and dried it at 212° ; after this I gradually heated it to redness, and found that it diminished

1.41 per cent. in weight. This experiment, slightly varied, was repeated with a difference of only 0.14 per cent. in the weight lost. It will be observed that although Mr. Cooper dried the precipitate at 300°, while I subjected it only to 212° before heating it to redness, yet I found the diminution of weight, caused by the subsequent and higher temperature, to be but little more than half of that which occurred in his experiment.

Supposing, therefore, the black precipitate to be a hydrate of platina, it would appear by my experiments to be a compound of about four atoms of the metal and one atom of water. Now the existence of a hydrate so constituted is not only of itself extremely improbable, but is rendered still more unlikely by the supposition that a metal without previous oxidizement should form a hydrate at all; for it is quite as contrary to experience that a metal and water should combine, as that a metal and acid should unite, without the intervention of oxygen.

I am therefore disposed to consider the 1.41 per cent. of water which remains with the black powder, after being heated to 212°, as in a state of mixture, and not of combination. There are other circumstances which strengthen this conclusion: if the black powder be strongly pressed or rubbed in a glass mortar, the metallic appearance of platina is as perfect before as after the application of a red heat; and I conceive this could scarcely occur if it were chemically combined with water. That the platina is in the metallic state in the black powder, is proved by its total insolubility in nitric or muriatic acid, even when first precipitated, and before it is dried. Another circumstance which induces me to believe that it is not a hydrate, is its answering most perfectly the purpose of spongy platina in firing a jet of hydrogen gas, and detonating a mixture of oxygen and hydrogen gases; indeed it appears to me to be an excellent preparation for these purposes, and it is procured with very great facility.

It is probable that some other metals whose affinity for oxygen is weak, may also be precipitated in the metallic state by the tartrates; and I have found this to be the case with gold: when tartrate of soda is added to the muriatic solution of that metal, no effect is produced until heat is applied, and then the precipitation of metallic gold is as rapid, and quite as remarkable as that of platina.

Tartrate of soda being a salt not usually kept, I have sometimes employed tartrate of potash: when the solutions are cold, the well-known double salt of potash and platina is precipitated; but on the application of heat the black powder is very quickly formed; the same effect is produced by tartrate of lime and tartrate of ammonia; but neither tartaric acid nor bitartrate of potash determine precipitation until an alkali is added.

It appears to me that muriate of platina may be very advantageously employed as a test of the presence of tartaric acid, provided it be first saturated or supersaturated with an alkali.

During the formation of the black powder, there is an evident evolution of some gas: suspecting it to be carbonic acid, I passed it

into lime-water, and obtained a plentiful precipitate; it is probable therefore that the hydrogen of the tartaric acid combines with the oxygen of the platina, and thus reduces it to the metallic state; whilst the carbon and oxygen of the tartaric acid form carbonic acid gas.

I am at present engaged in some researches upon the oxide of platina, thrown down by the action of protonitrate of mercury.—*Lond. and Edin. Phil. Mag.*, Feb. 23.

Action of Magneto-Electricity on the Tongue, by Mr. F. Watkins.—Among the several electrical effects exhibited by natural, artificial, steel, and electro-magnets, none are in my estimation more curious than that produced on animals. Dr. Faraday in his excellent paper on experimental researches in electricity, published in the *Phil. Trans.* of the past year, observes, in page 138, that when using an armed loadstone of great power, belonging to Prof. Daniell, he convulsed a frog powerfully, and adds with a diffidence so peculiarly his own, that “I thought also I could perceive the sensation upon the tongue, and the flash before the eyes.” That this eminent philosopher did observe the phænomenon in a slight degree (at this moment) there can be no doubt; but as the effect was feeble, in consequence of the exciting cause not being sufficiently powerful, he alludes to it in the manner here quoted.

Through the kindness and liberality of the proprietors of the National Gallery of Practical Science, Adelaide Street, West Strand, I have seen and performed several magneto-electric experiments with great success, with their superb artificial steel magnet; and as I have not noticed in print anywhere else than in Dr. Faraday’s paper, that the tongue had been electrified (if I may use the expression) by a magnet, I shall briefly recount what R. W. Fox, Esq., Mr. Saxton, and myself, experienced on the 5th of June last; and should you consider the remarks worthy a space in your forthcoming Number, they are at your service.

Two slight copper wires were so disposed, that while one had an end connected with the ascending portion of the compound helical wire surrounding the armature or lifter of the large magnet, the other was joined to the descending portion, thus affording the means of completing the circuit in the mouth. When the free end of one wire was situated underneath the tongue, and the free end of the other placed above that organ, and in contact,—on breaking the connexion between the armature and magnet, a shock was felt; and when the process was repeated several times, the sensation was truly painful. Hence the original observation of Dr. Faraday was perfectly correct.—*Ibid.*

Improvement in the quality of Iron and Steel, from their becoming rusty when buried in the Earth.—The following “extract from the Chronicles of Old London Bridge,” is sufficiently curious in

itself to merit insertion in the *Philosophical Magazine and Journal of Science*, and as an instance of observation, ingeniously applied.

An eminent London cutler, Mr. Weiss of the Strand, to whose inventions modern surgery is under considerable obligations, has remarked, that steel seemed to be much improved when it had become rusty in the earth, and provided the rust was not factitiously produced by the application of acids. He accordingly buried some razor blades for nearly three years, and the result fully corresponded to his expectation; the blades were coated with rust, which had the appearance of having exuded from within, but were not eroded, and the quality of the steel was decidedly improved. Analogy led to the conclusion, that the same might hold good with respect to iron under similar circumstances; so with perfect confidence in the justness of his views, he purchased, as soon as an opportunity offered, all the iron, amounting to fifteen tons, with which the piles of London Bridge had been shod. Each shoe consisted of a small inverted pyramid, with four straps rising from the four sides of its base, which embraced and were nailed to the pile; the total length from the point which entered the ground to the end of the strap being about 16 inches, and the weight about 8 lbs.

The pyramidal extremities of the shoes were found to be not much corroded, nor indeed were the straps; but the latter had become extremely and beautifully sonorous, closely resembling in tone the bars and sounding pieces of an Oriental instrument which was exhibited some time since, with the Burmese state carriage. When manufactured, the solid points in question were convertible only into very inferior steel: the same held good with respect to such bolts and other parts of the iron work as were subjected to the experiment, except the straps; these, which in addition to their sonorousness, possessed a degree of toughness quite unapproached by common iron, and which were in fact imperfect carburets, produced steel of a quality infinitely superior to any which in the course of his business Mr. Weiss had ever before met with; insomuch, that while it was in general request among the workmen for tools, they demanded higher wages for working it. These straps, weighing altogether about eight tons, were consequently separated from the solid points, and these last sold as old iron. The exterior difference between the parts of the same shoe led at first to the supposition that they were composed of two sorts of iron; but, besides the utter improbability of this, the contrary was proved by an examination, which led to the inference that the extremities of the piles having been charred, the straps of iron closely wedged between them, and the stratum in which they were imbedded, must have been subjected to a galvanic action, which in the course of some six or seven hundred years, gradually produced the effects recorded in the present paper.—*Philosophical Magazine and Journal of Science*, Jan. 1833.

BOTANY AND NATURAL HISTORY.

Influence of Coloured Rays on the Growth of Plants, by M. C. Morren.—In a paper which the author read to the Academy about two years since, he had shown, that of all the elementary colours, those which are most favourable to the manifestation and development of organized beings belonging either to the animal or vegetable kingdom, are red and yellow, and that this property exists in a nearly equal degree in each. These and other experiments were verified at that time only in the case of the most simple organized beings, in masses of water, subjected to the influence of the agents of the surrounding world. M. Morren has tried whether the same results would take place on making coloured rays act separately on earth in which some grains were put to germinate. The experiments were commenced on the 17th of March of this year. He took nine pots filled with well-dried earth, and of the same kind for each. In each pot he sowed twenty grain of cresses (*lepidium sativum*). These seeds were then covered with a bed of earth, three millimetres in depth. He sprinkled each pot with the same quantity of water from day to day. He covered each with a tin vessel, blackened both inside and outside, twenty-two centimetres in height, of a cylindrical form, one decimetre in diameter, shut superiorly by a plate of tin placed obliquely, and inclined at an angle of 45° . Each plate was perforated in the centre with a circular hole, before which was placed a circular pane of glass, four centimetres in diameter, and differing in colour for each vessel. These pieces of glass were such as are used in ornamenting the old windows of churches, and were all of the most beautiful tint: they were of the following colours, violet, blue, grass-green (*vert-pré*), sea-green, bright yellow, yellow, orange, red, purple. By the side of these vessels he placed another vessel, black, such as themselves, but with a white plate of glass. No ray passed through the solders, and care was taken to sink each vessel one inch and half in the ground. These were all placed on a shelf raised to one-half the height of a lightsome window. The fourth day of the experiment the radicles had shot out under all the vessels, and attained the length of from one to two millimetres. The sixth day it was observed that vegetation was much more advanced under the vessels than in the open air, and under the influence of the compound light. Under the yellow, and particularly under the bright yellow, the radicles were scarcely more developed than on the fourth day. Under the green rays there were some radical hairs towards their upper part which was somewhat yellow. The small plumulæ were yellow. Under the green rays the plumulæ were more yellow; the radicles and hairs were also as under the yellow rays. The orange, red, purple, blue, and violet rays corresponded to radicles of a centimetre yellow in the neck, to radical hairs of a millimetre, to plumulæ frequently curved and well formed. On the seventh

day the plumulæ were developed under all the vessels; they were very yellow. Under the white light they were becoming sensibly green; in the open air they appeared green. On the eighth day the shoots were from one to one and half centimetre in length; under the yellow rays they were not so long, white all over, the plumulæ yellow, the leaves the same, and bent back, the radical hairs two millimetres in length. Under the white light the shoots were scarcely three millimetres in length: they were becoming green, as also the leaves. On the ninth day there was an identity of character with respect to all the plants under the vessels: shoots of three centimetres, leaves of four millimetres, curved back very much, entirely yellow. In the air the shoots were scarcely a centimetre in length, the leaves very green. On the fifteenth day of the experiment, there was observed at length a strange difference with respect to the plants developed under the yellow rays; their leaves were become green, though paler than those of the plants in the open air. Under the orange rays a slight greenness presented itself. Under all the other rays the plants were evidently suffering, and were yellow.

From these researches the author concludes, 1. That in the same way as darkness favours the first period of germination, so also do the colours of the spectrum acting separately, possess a special influence which seconds this operation: but that among these colours those whose illuminating power (with the exception of green) is greatest, are also those which least of all favour the act which causes the developement of the rudimentary organs of the grain: 2. That under the coloured rays of the highest illuminating power, the radicles are developed least and most slowly: whilst on the contrary the plumulæ grow better and more rapidly; that under the coloured rays of a weak illuminating power, the radicles and plumulæ take on a developement similar to that which they attained in the dark, that consequently *the growth in length of vegetables under the rays of the prism, is in the inverse ratio of their illuminating power*: that under all the coloured rays, as in the dark, the radical hairs are developed towards the aerial part of the radicle, a sure sign of the growth, occasioned by each of these circumstances; that the lengthening of the organs proceeds under the coloured rays, as in the dark, and that the different parts grow much quicker there than under the influence of white light: 4. That the green colour of vegetables is developed much more rapidly under the influence of compound light, than under any ray whatever of decomposed light: that under all these rays the parts on the vegetable destined to become green, are yellow at first, then pass insensibly to the palest green, then to the deepest tint under such rays as enjoy the special property of suffering these changes to go on: 5. That these rays are on one side the yellow, and on the other side the orange: that the first possesses the *maximum* degree of this property, and the second the *minimum* degree, the other rays not at all producing the green colour: that the yellow ray promotes the green colour in proportion as it is less intense; but that it requires more time to produce greenness than

would be required in white light, and that it never can produce it in the same degree as the latter. 6. It is perhaps allowable to state, that this *viridificating* property of the rays in the spectrum arises from their illuminating power, and is intimately connected with it; but then it must be acknowledged that the green ray itself does not possess the property at all, though it shares with the yellow nearly in the *maximum* of illuminating power. In concluding his letter the author asks whether it is solely by its *splendour*, that light acts in the progressive colouring of vegetables, all the organic elements of which, though white at their formation, become subsequently covered with such lively and diversified tints.—*Annal. des Scien. Natur. Octob.* 1832.

Habits of the Ornithorhynchi.—Dr. Weatherhead communicated to the Committee of the Zoological Society several extracts from a letter which he had recently received from Lieutenant the Hon. Lauderdale Maule of the 39th Regiment, now in New South Wales. They referred to the habits and economy of the *Ornithorhynchi*.

“During the spring of 1831,” writes Lieut. Maule, “being detached in the interior of New South Wales, I was at some pains to discover the truths of the generally accepted belief, namely, that the female *Platypus* lays eggs and suckles its young.

“By the care of a soldier of the 39th Regiment who was stationed at a post on the Fish River, a mountain stream abounding with *Platypi*, several nests of this shy and extraordinary animal were discovered.

“The *Platypus* burrows in the banks of rivers, choosing generally a spot where the water is deep and sluggish, and the bank precipitous and covered with reeds or overhung with trees. Considerably beneath the level of the stream’s surface is the main entrance to a narrow passage which leads directly into the bank, bearing away from the river, (at a right angle to it), and gradually rising above its highest water-mark. At the distance of some few yards from the river’s edge this passage branches into two others which, describing each a circular course to the right and left, unite again in the nest itself, which is a roomy excavation, lined with leaves and moss, and situated seldom more than twelve yards from the water, or less than two feet beneath the surface of the earth. Several of their nests were, with considerable labour and difficulty, discovered. No eggs were found in a perfect state, but pieces of a substance resembling egg-shell were picked out of the *debris* of the nest. In the insides of several female *Platypi* which were shot, eggs were found of the size of a large musket-ball and downwards, imperfectly formed however, i. e. without the hard outer shell, which prevented their preservation.”

In another part of his letter Mr. Maule states, that in one of the nests he was fortunate enough to secure an old female and two young. The female lived for about two weeks on worms and bread and milk, being abundantly supplied with water, and supported her young, as it

was supposed, by similar means. She was killed by accident on the fourteenth day after her capture, and on skinning her while yet warm, it was observed that milk oozed through the fur on the stomach, although no teats were visible on the most minute inspection: but on proceeding with the operation two teats or canals were discovered, both of which contained milk.

The body of the individual last referred to (together with several others) has been preserved in spirit to be transmitted to Dr. Weatherhead, who stated his intention of examining it anatomically on its arrival, and of laying before the Committee the result of his observations on this interesting subject.

It was remarked, that the existence of milk in the situation described by Lieut. Maule is fully confirmatory of the correctness of the deductions made by Mr. Owen from the minute dissection of several individuals (including one in the Society's collection presented by Capt. Mallard, R. N., Corr. Memb. Z. S.), that the glands discovered by M. Meckel are really mammary. This opinion, with the anatomical reasons on which it was founded, have been lately laid by Mr. Owen before the Royal Society, in a paper which is published in the second Part of the Philosophical Transactions for 1832, and which has been noticed in the first volume of Phil. Mag. and Journal of Science, p. 384. Mr. Owen's dissections, however, though they established the existence of numerous minute tubes leading from the glands in question through the skin where it was covered by the wool, did not enable him to detect any canals so large as would appear to be indicated in Lieut. Maule's letter.—*Philosophical Magazine and Journal of Science*.

Inflammation of the Fraxinella. (Dictamus alba,) by M. Biot. —Among the physical phenomena which are produced during vegetable life, phenomena which might become a subject of curious study, there are few whose developement appears more surprising than that which is generally attributed to the fraxinella, viz., that of being surrounded in hot days, with a sort of ethereal atmosphere, which can be ignited by the application of a taper without injuring the plant. Such a phenomenon in fact would seem to require, that the inflammable vapour should be, as it were, prevented from expanding by the vital action, or else that its emission continually renewed, should always keep it dense around the plant, in proportion as it tended to expand in the external air, two states of things equally difficult to conceive. So singular a fact, however, is known only in a general way among botanists, without their having observed it themselves, and accurate details of it are to be found only in Deterville's *Dictionary of Natural History*, where Bosc thus says: "the extremities of the stalks, and the petals of the flowers of the fraxinella, are filled with an immense number of vesicles full of essential oil. On the hot days of summer they diffuse a strong-scented vapour, inflammable, and so abundant, that if, towards evening when a cooler air rendered it a little denser, we bring a lighted taper

near the fraxinella, there appears all on a sudden a great light which spreads over the entire plant, without injuring it." Chance having afforded me an opportunity of witnessing these phenomena of inflammation of the fraxinella, I determined to study their cause and physical conditions.

At first supposing, with those who have described the matter, the reality of an ethereal emanation which encompasses the plant, I instituted according to this view different experiments, but none with any success. I then directed my attention to the examination of the cortical vesicles, whence it was said that this inflammable atmosphere emanated. These vesicles, when observed with the microscope, present the form of small bottles, terminated by a sort of conical neck tapering to a point at its extremity. They have been very accurately represented by M. Mirbel in his "*Elements of Vegetable Anatomy and Physiology*." These are found distributed more or less numerously over every part of the stalk; they are seen in greater abundance on the peduncles of the flowers, principally in their lower surface, at the extremity where the flower is inserted; we may still follow them on the borders of the leaves of the calyx, on the borders and nerves of the petals, on the stamens and style; in fine, their grains more condensed thus cover all the surfaces of the ovaries, when they are enlarged by fecundation. Among these utricles some are sessile, others pediculated, the latter in different parts, but more frequently on the most vigorous. Very small at the commencement of vegetation, they become enlarged according as the plant increased. Their surface seen with a microscope in a strong light, exhibited itself commonly speckled with red and green, in the variety with the red flower; but it is all green in the white flower variety. The interior is filled with a colourless liquid, through which the light is refracted to a focus. I frequently saw at the extremity of this point a small limpid drop, as if a part of the interior liquid, dilated by increase of temperature, or secreted by the vital action, had flown out. These observations led me to think that the developement of the flame around the plant might be the result of simultaneous inflammation, or almost instantaneously propagated from these numerous utricles filled with essence. On this supposition the heat of summer was not necessary for the actual production of the phenomenon, but merely for the maturation of the inflammable liquid contained in the utricles; once the utricles were formed and ripened, the cold or heat of the time could not interfere, any more than the time of the day. The ignition must be effected merely by the contact of the inflamed body, or at least nearly by its contact in order to make the utricles burst. In fine, it must be accomplished with the characters of suction and of propagation suitable to small globules lying in juxtaposition, filled with an inflammable liquid, not with the instantaneous simultaneousness of a volume of gas. This is the mode of viewing the phenomenon, to which I have been led by all the experiments which I have made, some of which I shall here state.

The 26th of April, 1830, I tried to apply the flame of a match to

the peduncle of a flower of the red variety, which appeared to me already charged with a certain number of utricles considerably distended.

I did not obtain continued inflammation, but mere local crepitation, similar to those produced by jets of the essence contained in the orange peel, when pressed and held near the flame of a taper. The remainder of the plant where the utricles were smaller and fewer did not present even this phenomenon. I repeated the experiment the following year, and at the same time of the year, with similar results. In the parts where the crepitations were produced, the utricles appeared obliterated and blackened.

On the 15th of May, 1830, several flower stalks had acquired full developement: the utricles were considerably expanded and closely set on their surface. The entire day was cold and dry: in the evening the temperature being at 49° F. I repeated the attempt to inflame, the attempt succeeded when the flame was applied beneath the peduncles of some flowers fully developed, or only partly expanded, particularly near the commencement of these flowers, where the utricles are always more abundant. The inflammation though manifest, was not sufficient to pass spontaneously from the base of one flower to that of another; it was necessary to excite it at each point in succession, which I did with sufficient gentleness not to injure the stalks. Among those which presented the phenomenon, there were some which I had in vain tried the preceding April; some others, whose utricles having been inflamed were destroyed, might still after the lapse of a week be ignited anew, no doubt in consequence of other utricles have come to maturation since the preceding experiment. In the third attempt on the 22d May, the developement of the plant being more advanced, the inflammation was excited with great intensity over all the stalks. I have frequently since produced a repetition of the phenomenon on the same flower stalk at different periods; and having become more dexterous in conducting the process, I have been able to reproduce it seven or eight times this year in a sensible degree on the same stalk, by choosing successively its different parts to apply the inflammation to them. It is not necessary that the experiment should be made particularly in the evening any more than at any other time. In fine, the inflammation is always propagated from below upwards over an entire bunch of flowers, but with much more facility from above downwards: it may also take place on the peduncles of the centre without occurring on the lateral peduncles, though they may be in a fit state to receive the inflammation, by approximating the flame separately to their surface. This possibility of succession in the phenomenon of ignition, as well as of its insulation, is very easily understood for a system of globules separately distributed over all the parts of the plant, but it cannot exist for a continuous mass of inflammable vapour, such as that with which the fraxinella was supposed to be encompassed. The phenomena just described are produced on all the varieties of the fraxinella, whether the red-flower or white-flower variety, less easily, however,

and less abundantly on the latter, its utricles appearing smaller and fewer. It is known that external temperature, by modifying the progress of maturation considerably influences the absolute quantity of essential oil produced by the same vegetable. The cold constitution of this year seems to have thus acted on the phenomenon just described; the utricles of the *fraxinella* are smaller, and their inflammation appears less abundant than in preceding years.—*Annal. de Chim. Aug. 1832.*

ANATOMY AND PHYSIOLOGY.

Observations on the Blood.—(Extracted from a letter of M. Muller, professor of physiology, &c., at Bonn.)—I seize the present opportunity to communicate to the Academy an observation on the blood, which seems to me to merit some attention. I have found means to prove that it is not the globules which contain the coagulable part of the blood or the fibrine, but that on the contrary, the fibrine is dissolved in the serum. To prove it we must employ blood, the globules of which may be large enough not to pass through the pores of the filter: such is that of the frog. If, in cutting off the thigh of a frog, we let the blood flow on a filter of white paper, and mix it immediately with an equal quantity of water, or, what is better, with sugar and water, there passes into the watch-glass placed beneath a filter, a clear liquid, in which there is soon formed a coagulum of fibrine; this coagulum is at first as clear as water, so that it can be seen only by taking it up between the fingers—but it is soon condensed and becomes white. It is readily conceived that, in the little time which elapses before coagulation, a very small quantity only of dissolved fibrine can pass through the filter, and that the greatest portion of the fibrine is already coagulated on the filter. If instead of pure water we employ sugar and water, to facilitate the passage through the filter, the globules remain on the filter without undergoing any change, and are not dissolved. From this observation it follows, that the mode of explaining the coagulation of the blood, by the aggregation of globules, or of the nuclei of globules, is altogether without foundation.

These observations are not perhaps altogether undeserving the attention of the Academy; but it should be remarked that they cannot be made with frog's blood in winter, because this blood does not coagulate entirely during that season, and also that we cannot employ frogs that have been for any time taken, but it is the blood of fresh frogs only, that coagulates on passing out of the body; but during spring, summer, and autumn all fresh frogs, without exception, produce the phenomenon described. The globules of blood, besides, are composed, as we know, of a colourless nucleus and a red outside: the

latter is dissolved by little and little in water, (but not in salted or sugared water,) and pure water immediately changes the elliptical form of the globules of frogs' blood into the round shape. After the solution of the red outside in water, the nuclei remain insoluble in the water, but soluble in alkaline water. All these facts are easily observable with the microscope; it is a species of microscopic chemistry. In order to separate, immediately, the nuclei from the red outside, the best mode is to mix a drop of acetic acid with a drop of a frog's blood, or that of any other animal, making the observation with the microscope. The red outside, or the *cruorine*, is immediately dissolved in the acetic acid, whilst the elliptical nuclei remain, and may be observed in their proper form, which is elliptical in those animals whose entire globules are elliptical.—*Annal. des Scien.* Oct. 1832.

Case of Anencephalism.—A poor woman, aged 30, mother of two children, was delivered of a third in April, 1830; it was male and anencephalic, and yet it lived for 32 hours. Since that period, the woman had dreaded being again pregnant, as “she was certain (she said) that she should bear nothing but monsters.” On 4th July of next year, she was delivered of a female child; and this also was anencephalic. It lived for 20 hours. The respirations were feeble and distressing, and, soon after birth, the face and body became of a blue colour, which deepened more and more to a blackish hue. Its size was that of a full-grown and healthy child. The shape of the head was most extraordinary; the level of its vertex was on a line with the root of the nose—its dimensions were much contracted, as well from side to side as from before backwards; the face was fully formed, but the forehead and eye-brows were wanting; the eyes were startingly projecting, in consequence of the deficiency of the upper part of its orbit, and were situated at the most elevated points of the head; the inclination of the face was sloping backwards, so that the facial angle was much smaller than usual. On a careful examination after death, the whole of the vault of the skull was found wanting, and the open cavity was bounded by a rim formed by the frontal, parietal, and occipital bones; the basis of the cranium was much contracted in size in all its dimensions; the posterior wall was inclined forwards, so that the os occipitis appeared to rise vertically, in a line with the margin of the foramen magnum, and hence there were, in reality, no occipital fossæ; the anterior fossæ were scarcely distinguishable; there was no trace of the cribriform plate of the ethmoid bone; the os frontis consisted of two small pieces, joined to each other in the median line; and each piece consisted of two laminæ, one of which represented the orbital plate—the other the globular portion; the foramen opticum of the sphenoid was wanting; the petrous process of the temporal was fully formed, while the squamous plate was very imperfect; the parietal bones, in shape and appearance, were like a narrow band or rim along the upper edge of the temporal bones; the basilar process was placed vertically. All the vertebræ were perfect.

The scalp was covered with long hair, and extended somewhat beyond the bony margin of the cranium, and then became suddenly continuous with a red-coloured, thin membrane, which supplied the place of the vertex; it adhered to the subjacent parts, and had much the appearance of a cicatrix. On dissecting this off, a soft tumour, of the size of a walnut, and of a fibro-cellular tissue, without any trace of cerebral substance, was found to occupy the central part of the cranium; it adhered strongly to the dura mater in all directions: the cerebrum, cerebellum, and tuber annulare were quite deficient; the upper end of the medulla oblongata was to be seen, and it presented the appearance of having been cut from the tuber; it had no connexion with the fibrous substance described above. The spinal marrow was also quite normal. There was no trace of the optic and olfactory nerves in the cranium: in the orbit, the former was found, but very small, flattened, and reduced to an empty neurilema—posteriorly, it was lost in the dura mater; the eyes were large, and the retina perfectly developed. There was no trace of any of the cerebral nerves within the cranium, except of the trigemini, which appeared to proceed from a plexiform ganglion, situated in the fibrous mass; and, as this substance was infiltrated with blood, the nerves were with difficulty traced. The facial and auditory nerves were not attached to the medulla oblongata, but lay loose and unattached in the cavity of the cranium; on the outside of the cranium they seemed quite healthy; the auditory organs were fully formed. The spinal nerves were all normal in structure and arrangement. The carotid arteries were very small, and terminated in the fibrous mass; the vertebral were shrivelled to mere threads. Lungs nearly healthy: the ductus arteriosus and the foramen ovale were quite open. The abdominal viscera were perfect; meconium was found in the large intestines.—*Johnson's Journal, Jan. 1833.*

PATHOLOGY AND THERAPEUTICS.

Successful Case of Transfusion of Blood, by Doctor Schneemann, of Hanover.—The subject of this case was a stout healthy woman, aged thirty, who had already had two children, and at each delivery had suffered considerably from hemorrhage, before the removal of the placenta. On her third delivery, a violent hemorrhage set in about two hours after the birth of the child, the placenta being still retained in spite of the efforts of the midwife to promote its expulsion, by friction and pressure on the uterine region; she then attempted to extract it but in vain, she therefore sent at once for Doctor S. On his arrival, he found the patient in a faint, which had lasted for some time, and respiration and circulation were scarcely

perceptible ; the abdomen seemed pretty much enlarged, but the hemorrhage had ceased for the time. He immediately ordered her some wine and a tea-spoonful of tincture of cinnamon ; and when she had come a little to herself, introduced his hand into the uterus, and extracted the placenta, first removing the coagula which had quite plugged up the entrance. The organ then contracted powerfully, which of course prevented the renewal of the hemorrhage. The patient now got some more wine, and half a drachm of *secale cornutum* ; the latter being thrice repeated. In consequence, she gradually recovered, so much that Dr. S. did not think it necessary to remain any longer, having been already some hours with her.—Accordingly, he ordered her some laudanum, and went away. Soon afterwards, however, the husband of the patient came to him with the intelligence, that on his wife's turning in the bed, the hemorrhage had come on again with great violence ; that when he left her she was speechless, and that he feared she would be no more before they returned. Doctor S. at once saw that the only chance of saving the poor woman was to have recourse to the transfusion of blood.—Not having a proper apparatus for the purpose, he purchased a syringe with a long pipe, on his way to the patient's house, and brought two medical students with him as assistants. On their arrival, they found her with every sign of approaching dissolution : the hemorrhage had ceased, and the uterus was larger than when he had left her. He, therefore, again introduced his hand into it, removed the large coagula with which it was distended, and by pressing for a few minutes through its posterior wall on the aorta, endeavoured to determine the small quantity of blood that remained, more to the heart and the brain. By this means, together with pressure on the uterus from without, the organ began to contract, and resume its usual size and form ; leaving it to the midwife to attend to keeping it so, he next prepared for the operation of transfusion. The husband readily offered his arm ; and, after some difficulties, from the nature of the apparatus, about seven or eight ounces of blood were injected : the man then became so weak and faint that no more could be taken from him. In about half an hour after the operation, the woman began to come to ; and in three hours, with the assistance of wine and other restoratives, she was wonderfully recovered. The hemorrhage did not again return ; and though she subsequently suffered greatly from inflammation of the wounded vein, in consequence of which she had to undergo a severe salivation, she eventually regained her health and strength, a great paleness of the countenance being the only visible memorial of the danger she had escaped.

On torsion of arteries, Doctor Lorch, of Mentz, in a review on Delpech's and Jobert's cases of torsion of arteries as a means of obliteration, remarks that its failure in so many instances was owing rather to disease of the artery, or other circumstances unconnected with the operation, or to the imperfect performance of the operation itself. In proof of this he adduces the five cases reported by Amusat,

which were all successful, because the torsion had been skilfully performed. It is greatly to be wished that more extensive trials should be made on such an interesting subject.

Illusions in Maniacs.—M. Esquirol has deposited a memoir relative to the distinction of maniacs considered in a medico-legal point of view, and has read another paper entitled *Illusions in Maniacs*. The design of the author is to distinguish clearly in this new work hallucinations from illusions. In the former, every thing, according to him, goes on in the brain: the visionaries are persons who rave quite awake, and whose cerebral activity is so energetic, that it invests with substance and reality the images which are produced by the memory without the intervention of the senses.

In illusions, on the contrary, the patients are deceived with respect to the nature and cause of their sensations. Illusions are not all rare in the state of health, but reason soon destroys them; whilst in maniacs the case is not so. Two conditions, in fact, are necessary for the perception of a sensation: integrity of the organ which receives the impression, and integrity of the instrument which reacts on the impression. If the sensibility and activity of the organs are disturbed, the impressions made by external objects must be modified, and if at the same time the brain is diseased, it cannot rectify the error of the senses: hence arise illusions. The very volatile attention of maniacs cannot rest long on external objects, and then the preception is incomplete: the patients perceive but badly the qualities and relations of the objects which make impressions on them: in monomania, on the contrary, the attention is too much concentrated, and cannot carry itself successively over the objects which are external and foreign from the prepossessions and conceits which predominate over the patient's thoughts. In a word the mind and the passions concur with the senses in producing the illusions of maniacs: but it is from the senses that the process commences. Hypochondriacs have illusions arising from internal organs; they are deceived with respect to the severity of their suffering, but they are not actually bereft of reason, (*deraisonnement*) unless the case be complicated with lypemania or melancholy. M. Esquirol has examined the body of a woman at the Salpêtrière, who for a long time fancied she carried an animal in her stomach: she had cancer of this organ. An aged woman who was very devout, and who laboured under monomania, imagined that she carried in her abdomen all the personages of the Old and New Testament; when her pains became very severe she sometimes figured to herself that Jesus Christ was being crucified in her abdomen, and she said that she distinctly heard the blows of the hammer; when she was opened after death, they discovered the existence of chronic peritonitis, which caused extensive adhesions to all the intestines, so that they formed one mass. The same alteration existed, though less marked, in a demonomaniac who was extremely emaciated, who fancied that she carried in her abdomen several devils who were tearing her and inciting her to

self destruction. Her skin was as insensible as if it had been tanned, and M. Esquirol several times stuck pins into it without causing any pain. This woman stated that the devil had taken away her skin from her, and that he replaced it by his own. Irritation and pains in the organs of generation are oftentimes the cause of illusions in maniacs, particularly in women. The painful constrictions of the throat in hysterical monomaniacs are often attributed by them to the effects of some jealous person who wishes to strangle them. The wandering pains which maniacs sometimes feel in their limbs also give rise to illusions. A medical student, in an attack of mania caused by the presence of worms in the intestinal canal, felt acute pains all over the body, and attributed them to darts with which he fancied himself constantly pierced. The illusion went off after the expulsion of the worms. The author next passes in review the cases where the delusion arises from the external senses. The derangement of the digestive functions, and the perversion of taste almost invariably observed at the onset of mental diseases, often make the patients, who find fault with their food, fancy that they have been poisoned: a circumstance which contributes to inspire them with an aversion for those who have charge of them. This illusion disappears when the digestive functions are restored to their natural state. It is very important to distinguish this refusal of food from that which results from a fixed determination, as for instance from a vow, &c. The first has nothing alarming in it; the second on the contrary is very difficult to overcome. The dryness of their mouth, causes many maniacs to think that earth has been mixed up with their food, or that spoiled meat has been given them, &c. &c. After a very interesting examination of the illusions which arise from the alterations of the other senses, the author closes his memoir with the following conclusions. 1st, Illusions are the result of the actions of the sentient extremities, and of the reaction of the nervous centre. 2nd, Illusions are caused as frequently by the anomalous excitement of the internal organs, as by that of the external senses. 3rd, Illusions set reason astray with respect to the nature and cause of the impressions actually received, and excite the individual to acts more or less irrational. 4th, Sex, education, profession, habit, by modifying cerebral action, modify the character of illusions. 5th, Illusions assume the character of the ideas as well as of the passions which predominate in the persons affected. 6th, Illusions cannot be confounded with hallucinations, since in the latter, the brain alone is excited. 7th, Finally, reason dissipates the illusions of the man who is sound in mind, whilst it is unable to destroy the illusions of maniacs.—*Archives de Med.* Oct. 1832.

Medullary Fungus of Brain, by Doctor Hankel.—A woman, who from her infancy had been subject to epilepsy, the attacks of which returned almost every night, but who, in other respects, was not sub-

ject to any other serious affection, except that her intellects were rather limited, ceased to menstruate at the age of forty-four without any apparent bad consequences. Soon after she perceived a small tumour in the jaw. A year after, when Doctor Hankel saw the patient for the first time, this tumour had attained the size of a pullet's egg, was situated at the right horizontal ramus of the lower jaw, presented a smooth surface, and was red, firm, elastic—not painful nor moveable: the tongue still kept its natural position. Being afraid of an operation, the patient refused to allow any further examination. Six months after, the tumour became three times as large, filled the entire cavity of the mouth, pushed the tongue out of its natural place, was painful, and evidently divided into two parts.—The pain increased; deglutition became more and more difficult, and the patient died of inanition. Towards the latter period of her life the epilepsy disappeared altogether.

Examination of the Body.—On turning back the integuments of the cranium, we found on the right parietal bone an eminence some lines high, and to the extent of a fifteen sous piece: this eminence was soft and firmly attached to the pericranium. In the corresponding place within the cranium, we also discovered an eminence, which had made an impression, some lines in depth, on the surface of the brain: the dura mater did not adhere to this eminence as well as externally, with a firmly attached membrane, on the removal of which we discovered that this tumour was formed by a substance resembling the inflammatory clot of blood. By maceration, this soft substance was separated from the bone, and the latter was discovered to be studded over with points. The diploe had disappeared in this part, and the bone consisted of compact substance. Not far from this tumour a part of the cranium was perceived to be of a deeper colour, and somewhat depressed: it was found that there was a cavity situated between the two compact laminæ of the parietal bone, the exterior of which was a little sunk. We found, also, adhering to the pituitary gland, a small body, of a bright yellow colour, the substance of which resembled coagulated allumen. With respect to the jaw, we first found the bone luxated, and moveable towards its middle part: the tumour was divided into two portions, each of which was equal to the size of a fist; they appeared to have commenced their growth, one internally and the other externally, and were covered with a white, smooth membrane. The substance of these tumours was white, partly resembling lard, and in part reduced to the consistence of pap. The maxillary bone was destroyed in the centre, and we discovered some splinters of it, and points of bone in the tumour—which was also traversed in different directions by blood-vessels. The convex surface of the liver presented two white tumours, as large as nuts, and consisting of a white lard-like substance, covered by a soft white membrane. The other organs exhibited no alteration.—*Ibid. Dec.*

Employment of the Marchantia hemispherica in Dropsy, by Dr. Shortt.—In the treatment of dropsical affections it is of importance to

ascertain if the urine is changed by heat. If it coagulates, and is specifically lighter than natural, disease of the kidneys may generally be inferred; whereas if it continues clear, or becomes so from a turbid state on being boiled, and if its specific gravity is high, better hopes of relief or restoration to health may be entertained; since then there is less chance of the kidneys being organically affected. In the former case experience has pointed out the superior efficacy of blood-letting and *digitalis* liberally given; whilst in the latter mercury and squills have been found more beneficial, particularly when aided in obstinate cases by purgatives, sudorifics, and diuretics, such as *elaterium*, Dover's powder, the acetate of potash, sweet spirits of nitre, gin, decoction of broom tops, and cream of tartar, which, in curable cases, is probably one of the most valuable means we possess. In dropsies from imperfect absorption, where the action of the heart and arteries is languid and feeble, the mental and bodily powers depressed, accompanied by sallowness of the skin, even in the early stages of the disease, short dry cough, or breathlessness excited by the most trifling exertion, unaccompanied by disease of the heart, loaded tongue, and high-coloured scanty urine, these remedies require to be combined with tonics. The same treatment may also apply when the effusion arises from laxity of fibre or deficiency of the red globules of blood, as marked by whiteness of the complexion, with more clearness of the skin than in other cases; paleness of the lips; the mental faculties not being weakened in proportion with the powers of the body; the tongue being unusually clean, and at the beginning of the complaint there being neither cough nor breathlessness on making exertions and the urine being pale and watery. The anasarca, too, shifting from one part of the body to the other, according to posture, and pitting more deeply on being pressed than in other cases, forms a similar indication.

Dropsical affections, however, are symptomatic of so many different causes, that, notwithstanding the great variety of means in use for their cure, they are more usually fatal than other diseases, from long continuance in many instances, and from being generally connected with extensive organic visceral disease, producing obstruction to the free circulation of blood through the system at large. While such cases admit only of temporary relief, in others the fluid may be evacuated, and life may be prolonged by the proper employment and combination of remedies. But having been frequently disappointed in the treatment of dropsy by the unsuccessful results of the various means of cure in general use, and having often seen the deleterious effects of mercury and *digitalis* in some constitutions, more especially those with a strumous taint, I was induced to try the effects of liverwort, a remedy often beneficially employed in such cases by the lower orders in Ireland,—on accidentally hearing of its power as a diuretic in several remarkable instances from an individual who had personally witnessed its efficacy.

This plant, a drawing of which is annexed, consists of spreading

leaves of a leathery crustaceous matter, cut not very deep into lobes, entire about the edges, Fig. 4. It grows in almost every country, abounding chiefly, however, in moist and shady places, and on the banks of rivers, and is to be met with at all seasons of the year, but is supposed to be in its greatest vigour about the end of autumn.

I have now been in the habit of employing liverwort for several years in dropsical affections. In many cases it has been astonishingly successful; but it has, like other diuretics, failed. I cannot say, having frequently given it in decoction, I have seen the slightest benefit derived from its internal use; but employed externally in the form of poultice, I consider it of great value. The poultice is prepared by carefully picking and washing about two large handfuls of the leaves, these are thrown into a pot containing about a quart of boiling water, and simmered by the side of a fire for twelve hours, adding fresh water if required. It is then beat into a pulp, and as much linseed meal stirred in as to bring it to the consistence of a poultice, which is spread on flannel and applied to the abdomen, and fastened with a pretty tight bandage,—or it may be applied to the legs if anasarca of the extremities alone exists.

I have always repeated the application of a fresh poultice every twelve hours till the water is drained off; or continued it for such a time, as to show that no benefit may be expected from it, which will be ascertained in two or three days. The poultice produces in general copious perspiration, and at the same time acts powerfully on the kidneys. In some constitutions it occasions feelings of great sinking and exhaustion, but I have never known it do harm. No medicines of any kind have been given whilst the poultice is applied, unless the sinking feeling above described is distressing, when I found the *Spirit. Ether. Nitros.* in small doses, remove it; but its effects are increased by the patient's drinking plentifully of warm fluids; and I have always preferred weak beef-tea or chicken broth, with the view of keeping up the strength, which is generally at a low ebb in such patients. Opiates of all kinds I have found hurtful; but I employ warm clothing, and keep the patient in bed during the whole period in which the poultice is applied. I may also add, that it appears most successful in cases where other remedies have been extensively employed and failed, probably from the kidneys having been long previously excited; and judging by the state of the urine, when it is high-coloured, loaded, and depositing considerable sediment, which, on being exposed to heat, becomes clear; but I have also found it of service in cases where the urine coagulates from heat, and thus evidently showing disease in the structure of the kidneys. The following cases will show more decidedly its effects.

CASE I.—E. T. about 44 years of age, of a scrofulous habit, has considerable swelling of the abdomen, with fluctuation; has extensive eruptions over the arms and body; is greatly emaciated; and complains much of griping pains; bowels costive; pulse small and quick. Some time ago was under the action of mercury, of which she had ta-

ken at different times large quantities ; has since been freely purged ; urine very scanty, coagulable by heat ; has used the acetate of potass, and other diuretics, without benefit. On the 20th of September, a liverwort poultice was applied to the abdomen, which, during the course of the night, affected the kidneys. During the following nine days, seventy-four and a-half pounds of urine were discharged, being on an average of eight pounds daily. The poultice occasioning exhaustion, was then left off ; but the urine becoming again small in quantity, it was resumed on the 24th of October, and continued till the 12th of November,—a period of eighteen days,—when 196 pounds were passed, being an average of eleven pounds a-day. The poultice was again discontinued, but resumed on the 23d November, and continued at intervals a month, during which period 256 pounds were discharged, being on an average of eight and a-half pounds *per diem*, when all dropsical symptoms ceased.

CASE II.—J. A. is labouring under general dropsy ; the abdomen, arms, legs, scrotum, and penis, being severally distended with fluid. There is disease of the heart and liver ; pulse slow, irregular, and intermitting ; face livid ; breathing extremely difficult. Has been long under treatment ; and has taken a great variety of diuretics and purgatives, urine from three to four pounds. The *Marchantia* poultice was applied on the 3d August, from which period till the 21st,—being nineteen days,—250 pounds of urine were passed, being on an average of thirteen pounds a-day. At this period he was attacked with slight dysentery, from which he recovered, and the dropsical symptoms disappeared.

CASE III.—J. S. aged 50, took ill several days ago with swelling of the abdomen and inferior extremities, having had a bowel complaint for some days previously. Tongue dry in the centre ; much thirst ; urine coagulates by heat ; has no local affection ; nor can he assign any cause for his complaint ; has been in the habit of drinking much at times ; addicted at times to the use of spirituous liquors ; urine very scanty. After being freely purged, and receiving no relief from diuretics, the *Marchantia* poultice was applied ; and from the 27th of September to 26th of October,—being a period of twenty-nine days,—309 pounds of urine were discharged, being on an average of ten and a-half pounds a day, when he was dismissed cured.

CASE IV.—M. M. aged 30, of dissolute habits, and labouring under extensive organic visceral disease, became dropsical. Mercury, squills, digitalis, cream of tartar, and many other means, were tried without effect. She was completely relieved, however, of all dropsical symptoms, by the application of liverwort, about three years since, and is still living. In this case the secretion of urine had twice entirely ceased for upwards of seventy hours, accompanied with stupor and difficulty of breathing, which were not relieved by large bleedings, purgatives, blisters, &c., but on both occasions, the secretion of urine was restored by the application of liverwort poultices to the abdomen. The urine was coagulable by heat.

CASE V.—J. W. about 40 years of age, a labourer, after being

exposed to cold, became dropsical. Repeated diuretics and purgatives failed in relieving him; but after a few applications of the liverwort poultices, which occasioned violent perspiration, and considerable discharge of urine, he was permanently relieved.

CASE VI.—A. C. about 50 years of age, a labourer, of good constitution, but addicted to the use of spirits, became dropsical, and on hearing of the cure performed on J. A. applied the liverwort, and was cured in ten days.

Having requested Dr. Stephenson of the 13th Dragoons, to try its effects in India, the following cases were treated by that gentleman in that climate.

CASE VII.—P. M. a soldier in the 2d battalion of the Royal Regiment of foot, was labouring under dropsy. The abdomen was much enlarged, as well as the lower extremities. His case was considered hopeless by the medical officers of his corps, who had employed the usual means of cure without success. The application of the liverwort was followed by copious urinary evacuations, and he gradually recovered, having afterwards done duty as a soldier. He has since returned with his regiment to this country.

CASE VIII.—Sergeant P. of the same corps, of dissipated habits, and with liver affection, became dropsical. After the failure of other means, the liverwort produced marked benefit in his case, and on the regiment leaving India, he was so completely recovered as to remain in the country, having volunteered into another corps, and being received as fit for duty.

Many other cases of dropsy, illustrating the diuretic effects of the *Marchantia* poultice, I could have adduced; but have refrained from doing so, in order not to extend this notice beyond its due limits. It must be remembered, however, that the *Marchantia* is no more a specific than any other diuretic; but, as it has succeeded in many cases in which others have failed, I think it may be useful to practitioners to be acquainted with an agent, the mere external application of which is so effectual in exciting the action of the kidneys.

SURGERY.

Aneurisms, (By Breschet.)—M. Breschet has furnished an extract from three papers relative to aneurisms. The first treats of true aneurisms by dilatation of the arterial tubes; the second of mixed aneurisms, and the third of varicose aneurisms. Though the ancients have spoken of aneurisms, it is only however since the discovery of the circulation of the blood, that we have attained correct ideas with regard to this organic affection: still physicians are not even at this day entirely agreed with respect to true aneurisms, or those caused by simple dilatation of the arterial tunics. Scarpa maintains that aneurism always depends on a steatomatose alteration of the tunics, and since the publication of his work most pathologists have adopted this opinion. Others will have it that the

dilatation of the artery appertains only to the first period of the disease, and that the rupture of the tunics of the vessel constitutes its latter stages. However M. Breschet has ascertained the existence of a dilatation of the arteries which continues for the entire duration of the disease, and he has seen that this dilatation might be referred to four principal types according to differences in the form of expansion of the arterial tube. Thus he distinguishes, 1st, true *sacciform* aneurism; 2nd, true *fusiform* aneurism; 3rd, true *cylindroid* aneurism; 4th, true aneurism *by varix* (*en varice ou varicelle*.) This knowledge of true aneurism is not, says the author, a matter of mere curiosity; it is directly connected with the curative method proper to be employed. In *sacciform aneurism*, the vessel presents on one point of its circumference a tumour which may be compared to a small sac produced by expansion of the arterial tunics. This state considered in the ascending aorta might appear but a mere increase (exaggeration) of certain anatomical dispositions known to exist at the orifice of this arterial trunk or towards its substernal curve. We see above the sigmoid valves of the aorta, as well as towards the upper part of its curve, depressions or sinuses, which pathologists have considered as the first degree of an aneurism, but which their constant existence in adult subjects should induce us to consider as a natural and regular anatomical disposition. The large arterial tunics, such as the ascending aorta, the arch and descending aorta, are particularly the seat of the *sacciform* aneurism. However we meet it also on the carotids, the iliac arteries, and sometimes even on the arteries of the extremities. In this aneurism all the coats are simultaneously dilated: now, as the internal and middle tunic, particularly the latter, have but a very limited extensibility, it follows that we do not meet with *sacciform* aneurism, of a very large size; in general the size of these aneurisms may be compared to that of a filbert, or a nut. Yet we have seen some on the aorta which attained the size of a pullet's egg. In the *true fusiform aneurism*, the dilatation takes place over the entire circumference of the vessel: all the coats participate in it; the name by which it is designated sufficiently indicates its form. The calibre of the artery after having increased progressively for a certain portion of its course, then diminishes in a manner equally insensible till it returns to its natural size.

The *cylindroid aneurism* might strictly be considered as a variety of the *fusiform*; for we never observe here an abrupt transition from a given calibre to one much larger; it is no doubt for this reason that it has been almost wholly overlooked by pathologists; however it deserves to be considered separately. Accordingly we meet cases where a vessel is uniformly dilated for the length of one or two feet, the cylindrical form being perfectly preserved for its entire extent. This is observed on the arteries of the extremities, and on those of the cavities, particularly of the cranium. In these aneurisms the vessel assumes a calibre five or six times its natural size. The *cylindroid aneurism* differs from the arterial *varix* in this, that in the latter affection, at the same time that the artery is more or less dilated, it is

tortuous, and occasionally presents here and there small sacciform tumours, and the parietes of the vessel are thin, soft, and pressed together like the parietes of the veins, whilst in the other cases they are thickened rather than thinned. Usually when the arteries are increased in extent in one of their dimensions, they are also increased in the other. Still the lengthening which accompanies the dilatation in the cylindroid aneurism does not give to the vessel those tortuosities of the arterial varix.

The large arteries present the cylindroid aneurism more rarely than those of a middle or of a small size. G. Hunter, however, found in a woman the aorta from its exit from the heart to its passage between the tendons of the diaphragm, besides its increase in width become so much lengthened that it could not descend in a right line along the spine, as in the natural state, but exhibited frequent turnings in its entire course.

The cylindroid aneurism in the arteries of the smallest calibre, as in the capillaries, for example, is an aneurism by anastomosis. It approaches, in some respects, that kind of alteration which constitutes erectile tumours, which are the result of *aneurism by venous anastomoses*. It is distinguished from the latter in this, that the tumour which it forms presents pulsations isochronous to those of the pulse, whilst the other presents merely a transient turgescence, owing to the accumulation of venous blood in certain states of the circulation. The changes of colour, so remarkable in erectile tumours, are never observable in those which are owing to an aneurism by arterial anastomosis, and this is an additional mark by which to distinguish the one from the other. *Arterial varix* is a disease of the arteries, which may in every respect be compared to that of the veins from which it borrows its name. We observe a dilatation of the vessel in a greater or less portion of its extent, oftentimes through the entire length of the vascular trunk, and of its principal branches; besides this transverse dilatation there is a lengthening of the same vessel which becomes tortuous, and describes circuits more or less numerous and considerable. At times, independently of these abrupt dilatations of the entire arterial tube, we see on some points nodosities, or small circumscribed aneurismal tumours, which are true sacciform aneurisms, and sometimes mixed aneurisms. Most frequently the parietes are thinned, and softened; they collapse like those of varicose veins, whilst in the true cylindroid aneurism, the parietes have become thickened, and if they be divided perpendicularly to their axis, the orifice remains open. The artery affected with varix very much resembles a varicose vein, and one might mistake them, if injection and dissection, as far as the principal trunk, did not point out the real nature of the case. Besides, the distinction is always easily established by the pulsations of the varicose arteries. After having pointed out the different species of true aneurisms, the author deduces from these researches respecting the character of this disease, rules for treatment. He endeavours to point out the inutility of surgical

operations in this case, and the advantages to be expected from the employment of medical treatment. In the second paper, which is devoted to mixed aneurisms, the author sets about to establish, by precise observations, the existence of this affection, which consists in the destruction of the middle coat, in dilatation in the form of a pouch, and in the escape of the internal coat through the opening of the middle tunic of the artery. The third memoir contains a full history of varicose aneurism and of the mode of treatment which should be adopted. *Varicose aneurism*, which we should beware of confounding with *arterial varix*, results from the simultaneous lesion of an artery and a vein situated near each other, and is accompanied by an exchange of the blood proper to these two orders of vessels, through the opening at this point of contact. From the mixture of the black blood with the red blood in the arterial tube, there gradually results a dilatation of the artery, which, moreover, assumes all the characters of a vein. The parts to which the branches of the artery thus dilated by the entrance of venous blood into its cavity, are distributed being now less stimulated, inasmuch as the blood which they receive is partly venous, fall gradually into a stupor which may be compared to incomplete paralysis, as well with respect to motion as to sensation. In fine, we observe in this case effects resembling those of cyanosis, arising from the mixture of the two kinds of blood, in consequence of an anormal disposition of the heart. In proportion as this state of cyanosis becomes more manifest, the torpor increases, and the patient ultimately loses the use of the limb.

M. Breschet sets about to shew, that in order to obtain a cure in this disease, we should not confine ourselves to merely applying the ligature above the aneurism, according to Hunter's method, but that it is indispensable to include the communication between the two vessels between two ligatures. If in fact we do not embrace the artery above and below the diseased part, the blood may return by the lower end of the vessel, and thus reproduce the disease. It must be observed also, that by tying merely the artery, we only prevent the entrance of the red blood, whilst we afford a more easy access to the black blood, which alone circulates in the vessel, is distributed to the several parts, and necessarily augments the state of cyanosis and of torpor.—*Ibid.*

Case of Extirpation of the Parotid Gland, by Valentine Mott, M.D.—J. B. a native of St. Domingo, aged twenty-one years, came under my care in the latter part of June for a tumour situated on the face. He stated that he first observed it in January last, shortly after a severe attack of fever, and that it gradually increased in size until a few months ago, when he became alarmed at its progress, and decided upon visiting this country.

Upon inspection I found a very hard tumour, about the size of an ordinary fist, involving nearly the whole left side of the face, and evidently formed of the parotid gland, apparently in a scirrhus state.

From its magnitude, and as the only chance left for the recovery of my patient, I resolved upon attempting its extirpation, and with his free consent, after rendering the subject perfectly intelligent to him, fixed on the 13th of July for carrying it into effect.

Accordingly, on that day the operation was performed. It was commenced by interrupting the circulation through the external carotid artery by ligature, and for that purpose an incision was made from the posterior angle of the lower jaw downward and inward about three inches in length, so as to expose to view the inner margin of the sterno-cleido mastoid muscle. An enlarged lymphatic gland was now exposed, lying directly upon the sheath of the vessels. Upon turning it to the inside, the external carotid was laid bare, and tied immediately below the digastric muscle, and a little above the upper border of the thyroid cartilage. From the tumefaction of this part of the neck the artery was nearly three inches from the surface.

An incision was next commenced above the jugum temporale and carried downward in a semicircular direction, until it terminated upon the os occipitis. The incision in the neck was now extended upward, to intersect the one over the tumour.

On detaching the integuments in form of a double flap from over the diseased mass, its black appearance removed the impression of its scirrhus character, and fully demonstrated a melanotic condition of the gland. I however determined upon continuing the dissection, and proceeded to detach it from its various connexions. With this intention I commenced by dividing along the inner margin of the tumour the adipose and cellular tissue, until the inner edge of the masseter muscle was exposed to view. The finger was now introduced into the mouth, and cut upon in order to avoid dividing its membrane, and after separating the tumour for some distance from the masseter, to which it closely adhered, I detached it from the jugum, which had become more or less carious from pressure. It was next dissected from the mastoid and digastric muscles, and from the posterior angle of the jaw, but as the patient complained of excruciating torture when the tumour was raised from below upward, I determined to continue the dissection from above downward, and accordingly separated it, with a few rapid strokes of the knife, from the capsular ligament of the lower jaw, and removed the bulk of the disease. The portion filling up the space between the styloid and mastoid processes was cautiously detached with the handle of a scalpel, and the fascial nerve or portia dura divided by a quick movement of the knife. At the instant of the division of this nerve, he seemed to evince more pain than at any period of the operation. The muscles of the left side of the face were paralyzed. All the remaining portions of the disease were, as far as practicable, removed.

Several arteries were tied during the extirpation of the tumour, and after its removal. The trunk of the temporal was cut as it emerged from the disease, and yielded a profuse retrograde hæmorrhage.

The operation lasted about an hour, and the patient lost perhaps a

pint of blood. In the operation I was assisted by Drs. Vache, Wilkes, Hosack, and Dyhern, and it was performed in the presence of Drs. Harrow, A. Smith, Graham, and Seaman, and about twenty pupils.

After waiting a proper time, to see if any bleeding would occur, and refreshing our patient, although he did not seem exhausted, the wound was closed by several sutures and adhesive straps, and lint, compress, and double-headed roller, completed the dressings.

Evening. Has had a little sleep, and feels as well as he expected. No hæmorrhage; reaction has come on; took claret and water, with toast, which he sat up and eat with a relish, shortly after the operation.

14th, morning.—Left two pupils with him during the night, who report that he had slept well, and did not take the morphine that was directed, if he should be restless and in much pain. Pulse 94—skin natural. As he was averse to taking tea or barley-water, from the impression that it would occasion vomiting, he was allowed his favourite drink of weak claret and water.

Evening. Has had no evacuation from his bowels; directed the nurse to administer a common enema; pulse 104; skin pleasant; complains of considerable pain in swallowing.

15th, morning.—Enema operated favourably. Did not rest well during the night, but in the morning got some refreshing sleep. Skin a little heated; pulse 122.

Evening. Says he is more comfortable every way. Skin moist; pulse 108; recommended an enema.

16th, morning.—Has had a very undisturbed sleep, and feels better than at any time since the operation. Skin soft; pulse 96. Takes only sugar and water as a drink.

Evening. More comfortable than in the morning; has slept considerably during the day. Face more swelled; pulse 100; skin cool and soft; ordered an enema previous to bed-time.

17th morning.—Passed a pretty good night; had a free perspiration. Pulse 98; skin natural.

Evening. Feels in all respects better; pulse more frequent than in the morning; skin the same.

18th. Swelling of the face rather diminished; does not complain of any pain in the wound, or in his head; pulse 95.

19th. Some discharge from the lower part of the wound. Dressed it; looks very well. Pulse 88. Ordered chicken water and an enema in the evening.

20th. Says he passed an excellent night, and feels much better. Pulse 96; skin natural; swelling of the face subsiding.

21st. Dressed the wound; discharge very good; pulse 88.

22d and 23d. Dressed the wound; seems improving in all respects.

23th. Removed all the plasters, and dressed the entire wound; took away several of the sutures; very much healed by adhesion.

Pulse and skin natural; bowels have been moved naturally. Directed more nourishment.

27th. Upon cleansing the wound, ligature from the carotid came away. Removed remaining ligatures and sutures; wound mostly healed; at a small unhealed point opposite the lobe of the ear, the integuments appear to be taking on the melanotic aspect and a bloody fluid is discharged.

30th. Wound entirely healed, excepting at the point opposite the ear, which has every appearance of a reproduction of the disease. Complains of pain in the left knee, of which he has had several previous attacks. Directed leeches and warm fomentations.

August 15th.—Leeches have been repeated, followed by blisters. Pain in the knee less, but tumefaction increased; reunited portion of the wound taking on a melanoid fungous character; a small tumour making its appearance over the right eye-brow. Ordered syrup sarsaparilla, with a small quantity of mur. hydr. in solution.

17th. Several tumours have made their appearance upon the scalp; fungous of the wound rather increased; a dark spot showing itself in the integuments of the diseased side of the face; tumefaction of the knee increased; complains of pain in the right side. Skin assuming a yellow colour.

20th. Tumours all increased in size; hepatic affection very decided; confirmed hectic, and is evidently sinking. Every thing done to make him comfortable.

31st. In all respects worse. Permitted every thing he desires.

Sept. 5th.—Died this morning.

After death every exertion was made in order to obtain an examination of the body, but such were the feelings of the friends of the deceased, that even the desire of passing a lancet into one of the tumours on the scalp, with a view of ascertaining their positive character, although I had no doubt of the case being constitutional melanosis, was obstinately refused, and compelled to be abandoned.

On dividing the tumour longitudinally, not a vestige of the original organization of the gland could be observed. The inner surfaces had the appearance of firm tar, and imparted a black colour to the fingers when touched.—*American Journal.*

Partial Amputation of the Foot.—Baptiste L'Hote, thirty years of age, of the middle size, and of a lymphatic temperament, apparently of a scrofulous habit, a weaver by trade, sprained his left foot in running in the month of February of 1825. Immediately after the accident there came on considerable swelling, for the relief of which the most insignificant remedies were employed. In the month of March following, pus appeared above the third cuneiform bone. On the 7th of April, of the same year, this patient came to consult me: the foot at this time exhibited considerable inflammation and swelling. I enlarged the opening, through which a considerable quantity of healthy pus discharged itself. I prescribed leeches, emollient cataplasms, warm baths, absolute rest and regimen. From this time

the patient left me to place himself under the care of an officer of health (*officier de santé*), who promised him a radical cure, on condition that he would consent punctually to take the medicine of Leroy for a year. Two years passed without the least change appearing, except that the ulcer shifted its place. It at first appeared over the two first cuneiform bones, which it subsequently left, and settled on the middle of the first metatarsal bone, where it continued for the three last years preceding the amputation. Pus flowed continually in large quantity, and diffused an intolerable stench: the pains were very acute at intervals; there were frequent hemorrhages through the sore. The patient continued in this wretched state for two years, without being able to work, and could walk only on crutches.

On the 20th of August, 1830, I was again consulted. I observed the following symptoms: extreme emaciation, pulse weak and rapid, loss of appetite and of sleep, continued fever with excessive thirst: the left foot presented considerable œdema without redness: it was also the seat of acute lancinating pains, which frequently extended up along the leg: an ulcer, the edges of which presented nothing remarkable, about the size of a frank-piece, occupied the inner part of the foot, corresponding to the middle of the body of the first metatarsal bone. Desirous to ascertain exactly its depth and direction, I found it impossible to do so: the presence of the probe occasioned acute pain and hemorrhage, notwithstanding I used the greatest precautions. I determined to make an incision with a bistouri and a probe along the internal border of the foot, from the ulcer to the part immediately over the first cuneiform bone. By this means I obtained a tolerably good idea of the state of the parts, and became certain that caries engaged several of the bones, on which it already made great ravages. After examining this matter very attentively, I proposed, as the only means of cure, partial amputation of the foot; the patient acceded at once to my proposal, and appeared to evince considerable courage. I prescribed diluent drinks for him in large quantity. The amputation took place on the third day according to the following method.

The patient being laid on an elevated table, the extremity of the left leg was made to project considerably beyond the table, and the limb was held by a strong and intelligent assistant: with the left hand I held tight the integuments on which I was going to operate; then with the right hand holding a knife (*couteau interosseux*) of middle size, I made two longitudinal incisions in the skin, one to the upper side of the first metatarsal bone, and the other to its internal side. They commenced at the bifurcation of the toes, and were continued as far as the articulation of the scaphoid with the first cuneiform, where they united. I dissected off two flaps; the lower much larger and thicker than the upper, was intended to cover the second metatarsal bone through its entire extent, and a portion of the second cuneiform bone which I thought exempt from caries. I exposed the articulation of the scaphoid with the first cuneiform bone by a small transverse incision on its dorsal aspect. The two flaps being turned

back I took hold of the large toe with the left hand, and I placed the knife between it and the flesh of the internal border of the foot: my instrument was soon arrested in its course by the first metatarsal bone, this bone being divided by the caries into two nearly equal parts, the lower being placed obliquely on the second metatarsal bone. Having approximated the first toe to the others, the knife passed on without difficulty. The first cuneiform bone being removed, I beheld with astonishment that the second was also attacked with caries. I hesitated for some seconds whether I should conclude the operation, according to Chopart's method, or only remove the second cuneiform bone. I adopted the latter plan, notwithstanding it presented several difficulties, as will appear evident to any one who considers the anatomy of the parts. The transverse incision was enlarged, and the diseased bone exposed. All the parts uniting it to the scaphoid, the second metatarsal and the third cuneiform were divided with the point of the knife. This part of the operation was tedious and difficult. My operation being thus concluded, and there being no necessity for ligatures to arrest hemorrhage, I cleansed the wound, covered the cartilages of the bones, which I had taken every pains to spare, with fine lint; approximated as much as possible the two flaps which I kept together by four sutures. The entire was covered with an appropriate bandage. The wound united by the first intention for the greatest part of its extent particularly towards the toes, and suppurated at the other points. The fever following the operation presented nothing remarkable; pus became healthy and abundant. The wound was carefully dressed once a day up to the 17th of September. A perfect cure was effected by the commencement of the month of October. Since then all the motions of the foot are perfectly free; progressions is performed without lameness. The first of the toes remaining is much shorter than the others, as may be readily conceived.

Since this operation took place I consulted a considerable number of surgical treatises ancient and modern, and I have not been able to find any case recorded, where the two first cuneiform bones were removed, and the second metatarsal still preserved. It is to Chopart that French surgery is indebted for partial amputation of the foot at the articulations of the astragalus with the scaphoid. Since his time several eminent surgeons, particularly Lisfranc, have performed partial amputation; some recommended the amputation at the carpo-metatarsal articulation, whilst others adopt Chopart's method. No matter which of those two methods be adopted, the patient must lose the greater part of his foot, an occurrence which did not take place in the case which has been here related.—*Arch. Gen.*

THE
DUBLIN JOURNAL
OF
MEDICAL AND CHEMICAL SCIENCE.

1 MAY, 1833.

PART I.
ORIGINAL COMMUNICATIONS.

ART. X.—*Observations on the Treatment of Various Diseases.* By ROBERT J. GRAVES, M.D., M.R.I.A., King's Professor of the Institutes of Medicine.

(*Continued from Vol. II. page 180.*)

ON THE HEADACHS OF YOUNG WOMEN.

No cases prove more troublesome to the practitioner, and for none is he more frequently consulted, than the headachs of young women. The treatment of this affection, when it arises from an obviously plethoric habit of body, frequently attended by constipated bowels, is sufficiently well understood, and the physician feels pretty confident of giving relief by prescribing early hours, spare diet, and active exercise, together with the occasional exhibition of rather powerful purgatives. When the determination of blood to the head is very violent, such constitutions bear loss of blood well, and accordingly leeches may be applied behind the ears, or to the feet: when applied in the latter situation, the bleeding can be easily promoted by keeping

the feet in hot water, and I think that this method is even more efficacious than the application of leeches to the head, or its immediate vicinity ; occasionally immersing the legs as far as the knees in water as hot as can be borne, will relieve the headach. The effect of hot water thus applied to the lower extremities, on the general circulation, is familiar to all, and was exemplified in a striking manner in the case of an old gentleman subject to attacks of violent palpitations, accompanied by the feeling of approaching dissolution. I was sent for, during the absence of his attending physician, Doctor Thomas Beatty, and found him in one of those paroxysms ; it had lasted many hours, much longer than usual, and a fatal termination was expected both by himself and his friends, as the remedies which usually gave him relief had been tried in vain. By the use of a pediluvium as hot as he could possibly bear it, the palpitations and the sensation of mortal anxiety under which he had laboured ceased in a few minutes, and he lay down, took some nourishment, and had a refreshing sleep, from which he awoke quite recovered. In explaining effects so striking, we must not merely confine our attention to the fact that the pediluvium restores the active circulation of the lower extremities, but must recollect the extreme nervous sensibility of those parts, particularly the soles of the feet : no part of the surface of the body possesses so exquisite a degree of feeling, and hence none is better calculated for being the medium of receiving impressions from cutaneous applications. In most persons the immersion of the feet in water even moderately hot causes a powerful impression, and often a passing sensation of nausea. When cold water is used, the general circulation is visibly deranged and respiration somewhat affected, as may be seen in the case of persons walking into the sea ; and it is worthy of remark that this impression of cold on the feet acts likewise on the alimentary canal, as is exemplified in the immediate good effects occasionally experienced in cases of colic with obstruction, from causing the patient to walk with bare feet on cold flags, a mode

of proceeding at times also effectual in promoting the evacuation of urine in spasmodic dysuria. I dwell on those facts, because there is in certain constitutions a close connexion between cold feet and headaches, the former appearing in many cases to aggravate or even induce the latter ; in ordering applications to the feet in such cases, whether in the form of simple or medicated pediluvia, of sinapisms, or of frictions, the scientific physician will be guided by a knowledge of the extensive sphere of action such local applications enjoy.

In the habitual headaches of robust and plethoric young women, it is sometimes necessary to have recourse to general blood-letting, when the paroxysm is violent. Thus, in the case of a young lady, seen by Dr. Cheyne, Dr. Marsh, and myself, in consultation with Dr. Stokes, the paroxysms of headache were of most distressing severity, and had baffled for years all internal remedies and external applications, nor were they at all relieved by the means we recommended as the result of our consultation ; after repeated attacks, Dr. Stokes bled her *ad deliquium*, during a violent paroxysm of headache, and with immediate relief ; and it is very remarkable that the relief was permanent, for she has not since been attacked ; where a suppression of the catamenia occurs in such persons, it of course aggravates the headache, and in many instances it is the sole cause of it ; indeed this applies to all cases of headache occurring along with suppression, and therefore it may be well to offer a few remarks on the most effectual method of restoring the menstrual evacuation.

The periodicity of this function can still be traced, even in cases where suppression has continued for a great length of time by means of the menstrual *molimina*, which occur at stated intervals ; in endeavouring to bring on the discharge, therefore, we must be guided as to the time the attempt should be made by an observance of the period at which these *molimina* occur ; for a few days before that time, our efforts to produce a determination of blood to the uterine may be judiciously employed, and if they fail, the attempt should be abandoned, until a few

days before the next menstrual period: of course, I speak not here of the general constitutional treatment, for this must be constantly persevered in, one of the chief means of bringing back this evacuation being the restoration of the health to the natural standard; in some this is to be effected by tonic, and in others by an opposite mode of general treatment.

But of this it is quite unnecessary to speak, as all practitioners are acquainted with the essential difference between the general modes of management required according to the constitution and habits of the patient. What I wish to impress on the minds of the junior members of the profession is, that all those remedies which actually determine to the uterus or its neighbourhood, as pediluvia, stuping of the genitals, leeches to the inside of the thighs near the labia, aloes and other stimulating purgatives, &c. &c., should be only used at the times already spoken of. To use them at any other period, either after the menses have disappeared, or during the intervals between them, tends in most cases still further to derange nature, by determining to the uterus at an unseasonable time, when there is no natural tendency to that organ; under such circumstances the very same means will frequently fail and prove injurious, which, applied so as to coincide with the time of the natural effort, would have been successful. To illustrate these principles by an example: we are consulted in the case of a young woman, affected with various hysterical symptoms for several months, and during that period more than usually subject to headach, languor, loss of spirits, diminution of appetite, and irregularity, usually constipation of bowels; she is pale, and complains of various pains and uneasy sensations, and has not menstruated since the accession of these symptoms; here it is evident that the constitutional treatment must be strengthening and tonic; the practitioner will therefore recommend regular hours, much gestation in the open air, a nutritious diet, tepid and afterwards cold shower baths; he will regulate the bowels and afterwards prescribe a course of

tonic medicines, chalybeates, preparations of bark, strychnine, &c. &c. ; he will likewise inquire carefully when the last period happened, and when and how often since that occurrence menstrual molimina were observed. He thus ascertains when they should again recur, and contents himself with enforcing the constitutional treatment, until about six days before the calculated time. Then he lays aside the other medicines, and has recourse to those means which determine to the uterus. Two leeches are applied to the inside of the thigh near the labium, every second night, until they have been three times applied. The bleeding is encouraged by stuping. On the intermediate days the bowels must be actively moved by aloetic pills, and for three nights before and after the day of the molimina,* hot pediluvia, rendered stimulating by mustard seed, may be used ; during the same time also frictions with stimulating liniments should be applied to the feet and legs every morning, and spirits of turpentine or tincture of cantharides may be exhibited internally, while the necessity of more active exercise is inculcated. The intention of the leeching is to produce a tendency of blood to the part, which tendency is increased by each repetition of the application, and is still further augmented by these applications being made only about the time that the menstrual discharge should have taken place. *If these means fail, they must for the moment be laid aside, and the constitutional treatment must be again resumed until the same number of days before the next period, when the list of remedies above spoken of must be again tried,* and in few cases indeed shall we find them to fail. This periodic application of means calculated to determine to the uterus, at the very times that the efforts of nature are directed to the same organ, I have found

* By *molimina* are meant pains in the loins, thighs, and hypogastric region, flushings, colicky pains of the abdomen, increase of headach, and a general feeling of mal-aise, which are familiarly known among females as indicating a constitutional effort.

most successful and satisfactory. It is true that the catamenia may, and in hundreds of cases are restored by medicines exhibited at random with regard to the periods; but there is no doubt that their re-appearance can be effected with much greater certainty in the way I have pointed out, and if I am not mistaken, their re-appearance at the natural period has a more salutary effect on the constitution, than if they had been forced to come on at other periods. This rule of practice is perhaps not new; it is not proposed as original, but I am anxious to put it forward strongly, because daily experience proves that it is disregarded by the majority of the profession. With regard to the application of leeches to the thighs, I have mentioned two as a fit number in weakly habits, when the constitutional treatment must be strengthening and tonic; it is right to observe that in plethoric young women, in whom a contrary mode of constitutional treatment is proper, four or even six leeches at a time may be used with advantage.

The restoration of the menstrual evacuation to its proper period and quantity, will in many cases be in itself sufficient to relieve the tendency to headachs. In some young women, however, this tendency may exist from the beginning without any menstrual derangement, or may, along with many hysterical ailments, be caused by leucorrhœa, for which the best remedy is nitrate of silver in solution, used as an injection, as recommended by Mr. Jewel. Where leucorrhœa exists, the use of pediluvia often increases the evil and must therefore be avoided. In young women, leucorrhœa causes, as is well known, a series of most distressing symptoms, and therefore whenever headach is complicated with this we must remove the latter, as the first step in the cure. Having briefly sketched the treatment to be pursued in headach, as it occurs, first, in plethoric young women, secondly, in cases of suppressed menstruation, and thirdly, in cases of leucorrhœa, I must next speak of headach in young persons of a delicate excitable temperament, without any menstrual or leucorrhœal complication; such persons are said by

their friends to be extremely nervous, and are subject to every variety of hysterical seizures, all however marked by the violence of the accompanying headach ; no matter what be the form of the hysterical seizure, whether it be fits, trance, or catalepsy, the permanent symptom is headach, and that proportioned to the violence of the seizure ; it is of the pain in the head they complain, when able to express themselves, and they all feel convinced that it is the chief cause of their other sufferings. In some this pain is attended with flushed countenance, in others, the external signs of cerebral congestion are less evident, but in all the true cause of the headach is rendered evident by its immediate aggravation, if wine, even in the smallest quantity, be administered in order to counteract the alarming state of debility to which such patients are frequently reduced. It is this pain in the head which often keeps such persons awake night after night, and which, in habits such as I have described, the physician finds so extremely difficult to remove, for it is evident that most of the means usually resorted to for the cure of determination of blood to the head, must act very injuriously upon constitutions so delicate and possessing so little stamina. Such persons bear active purgation very badly, and loss of blood, whether general or local, infallibly increases the constitutional excitability and weakness ; it is true that much temporary relief often attends the application of leeches to the temples, and the headach occasionally ceases altogether, and usually it at least considerably diminished while the blood is flowing from the leech-bites, and for a short time afterwards. But this relief never lasts more than a few hours, and indeed often ceases very shortly after the actual bleeding has been stopped ; and we then have the mortification of finding our patient as much tormented by the headach as ever, while she is at the same time considerably weakened by the loss of blood ; indeed it may be laid down as a rule of practice, applicable to other parts as well as to the brain, that in debilitated, nervous, and hysterical females, however violent the congestion of an organ may be, the attempt at

curing this congestion by either general or topical blood-letting is injudicious, for when the constitution recovers from the immediate effects of this treatment, it will be found more disposed than ever to give rise to congestions usually of the same, occasionally of some other part.

The truth of this principle is strongly confirmed by the effects of blood-letting either general or topical in cases of epilepsy, *in weak and nervous habits* ; and yet no disease is more manifestly dependent on a state of cerebral congestion than the epileptic fit ; detraction of blood is sure to relieve the violence and shorten the duration of the fit, but it is as sure to increase the subsequent tendency to their recurrence. Thus a lady, concerning whom I was consulted by Mr. Kirby, had been liable every third or fourth month to a violent fit of epilepsy for the last twenty-five years ; about a year ago a young practitioner imprudently used the lancet, and she has since been subject to an attack every third or fourth week !

These observations would be protracted far beyond their intended limits, were I to relate the various cases illustrative of this truth I have witnessed, and often witnessed with pain, on account of the injurious consequences that resulted from its being either unknown or overlooked. In such cases where much suffering is complained of, particularly in so important a part as the head, the practitioner, whose attention is forcibly drawn to this prominent feature of the complaint, both by the patient's suffering, and the representations of her friends, is too apt to be led away by the temptation of affording striking and immediate relief of this particular symptom ; he applies leeches, the headach returns in a few hours, and leeches are again applied in increased number, and perhaps repeated a third time, until the debility is so alarming as to induce him to stop. What is now to be done ? The young lady's head is shaved, the scalp and perhaps the nape of the neck are blistered, or else cold lotions and bladders full of ice are applied to the shaved head, and in short this nervous and delicate female, labouring

under hysterical congestion of the brain, is ruthlessly subjected to the same severe discipline and remedies that are required for the treatment of actual phrenitis; nay, in two cases, I lately saw salivation induced, I need scarcely add with great subsequent injury to the constitution of the patient; let me again repeat that this headach and cerebral congestion are sometimes accompanied merely by debility, watchfulness, and repeated attacks of common hysterical convulsions. In other females the convulsions are accompanied by a peculiar trance-like state, in which the patient when not agitated by the convulsions, lies tranquil and quiet, the eyes being open, but she is totally unable to speak or move, and her perceptions and memory are extremely imperfect. In others, again, the convulsive movements gradually cease, the eyes are closed, and the patient appears to be in a comatose state; she hears, however, and can whisper a few short words intelligibly.

To treat this affection properly it is necessary to bear steadily in mind, that its natural tendency, when art does not interfere, is by no means dangerous. It is true, that the patient's state appears very alarming, particularly when many other anomalous symptoms affecting the stomach and bowels accompany those already described: still, however, in the weakly and delicate, and at present, my observations must be considered as entirely confined to such persons, the danger from the cerebral affection, however violent, is but trifling. To remove it, however, as speedily as possible, without irritating, weakening, or otherwise injuring the patient, is an object of great importance. In such cases I never bleed, never leech, never order the head to be shaved, nor do I ever blister. The means to which I trust are, first, moderately cold applications to the forehead; secondly, attention to the bowels by means of foetid and terebinthinate enemata, at least once a day; thirdly, attention to the state of the bladder, lest water should accumulate, as it frequently does in that organ; fourthly, extensive, diligent, and frequently repeated dry cupping of the integuments in the vicinity of the

head; fifthly, the internal exhibition of spirit of turpentine in considerable doses; sixthly, the repeated use of stimulating linaments to the abdomen and the lower extremities; and, lastly, when the fit has subsided, or other remedies have failed, the *nitrate of silver*, in considerable doses.

The utility of both nitrate of silver and spirit of turpentine in such cases, was suggested to me by the good effects these medicines are found to produce in epilepsy, particularly when it occurs in persons of a nervous and delicate habit, and since I have employed them in hysterical determination to the head, I have been able to overcome these and similar affections, with much greater facility than formerly: of these, as has been already observed, the spirit of turpentine is best suited to the violent stages of the disorder, and may be given in doses of one or two drachms, to be repeated according to its effects. The best vehicle is cold water; some will bear and derive advantage from two or three doses of this medicine in the day, experiencing from its use a diminution of headach, and removal of flatulence, together with a moderate action of the bowels and kidneys. In some cases, as occurs also occasionally in the treatment of epilepsy, by this medicine, it cannot be persevered in, in consequence of the violent dysuria and hæmaturia it occasions; slighter degrees of these affections should not, however, prevent our continuing it. When the paroxysm has abated, or when the spirit of turpentine has failed, the greatest benefit may be derived from the nitrate of silver continued for five or six days at a time, in doses of half a grain four times, or even six times a day. When the bowels are constipated, there is no better combination than nitrate of silver with minute doses of compound colocynth pill, a formula, I believe first recommended in dyspepsia by Doctor James Johnson, of London, and which I have found invaluable, not merely in the headachs of hysterical young women, but in those of men, particularly the habitual stomach headach, to which delicate and literary men are so subject.

In a lecture delivered at the Meath Hospital, and which

has been published in the London Medical and Surgical Journal, I have fully explained my views concerning the utility and extensive applicability of dry cupping in many diseases where loss of blood cannot be borne, and must beg leave therefore to refer to that lecture, together with the observations of Mr. Robertson in the *Lancet*, and of Dr. Arnott in his work on Physics, for an explanation of the *modus operandi* and effects of dry cupping, as a remedy. With reference to the present subject, it is sufficient to remark that in cases of hysterical headach, coma, &c., several, often so many as six cups, should be fastened on at once, to the nape of the neck, between the shoulders and below the clavicles; these cups should be all tolerably large, besides which, one or two small ones may be applied near the ears. The suction should be powerful, and should be sufficient to fix the cup for at least ten or fifteen minutes. In a young lady in Grafton-street, in whose case I first tried this method, its good effects were most striking; she had been lying for twenty-four hours, with her face somewhat swollen, her eyes open and unmeaning, unable to speak, and frequently agitated by violent hysterical convulsions. Mr. Moore, of Anne-street, applied the cups, and after they had been some time on, she recovered her consciousness and was able to speak. This result was the more remarkable, as she had, a year before, laboured under a similar but less severe attack, for which she was treated, by two of the most eminent practitioners in Dublin, by means of shaving her head, leeches, ice, &c. &c., a mode of treatment which left her in so weakened and nervous a state, that her removal to the country became necessary, and she did not recover her usual strength for several months. The gratification of her friends therefore on the present occasion, was very great indeed, at finding much more decided and speedy relief effected without the necessity of resorting to the remedies employed in the former attack. In epilepsy it may be easily conceived that dry cupping applied to the neighbourhood of the head, may afford considerable relief, and so in fact it does, and is most useful in averting

the paroxysms, particularly in those cases where previous head-ach, or other premonitory symptoms, advertises the patient of the approach of the fit, in time to have recourse to this application. Its good effects have been well illustrated in a young lady thus affected, and concerning whose case I was consulted by Mr. Halahan, of Stephen's-green. I may observe, that there is a species of hysterical delirium, attended by great nervous excitement, sleeplessness, talkativeness, and delusions, such as supposing persons to be present who are not so, accompanied by a frequent wish to get out of bed in some, while others hide themselves under the clothes when a stranger approaches. I say, in such cases I have known the most disastrous consequences result from the depleting system being solely relied on; in such cases the dry cupping, as before recommended, would probably prove a most valuable auxiliary to a well directed internal treatment.

Mr. Barker, of Britain-street, who had formerly frequent opportunities of witnessing the effects produced by dry cupping, has given me the particulars of a very curious case: a lady of rank, living in the vicinity of Dublin, was occasionally attacked by violent determination of blood to the head, and each of these paroxysms was sure to induce before it ended a violent propensity to suicide, which she very nearly succeeded in gratifying on more than one occasion. This propensity and the cerebral congestion which caused it, were afterwards removed, or rather prevented, by the timely application of dry cupping, as soon as the well known premonitory symptoms of the paroxysm made their appearance.

EXHIBITION OF OPIUM IN THE FORM OF ENEMA.

Dupuytren has recommended the exhibition of opium in the form of injections, in preference to the usual method of giving it by the mouth. The two following cases exhibit striking proofs of the utility of this practice, and its great superiority over the common method.

J. B. aged 30, by profession a Surgeon, was admitted into Sir Patrick Dun's Hospital on the 8th February last, in an extreme state of emaciation and debility, in fact, a complete skeleton, and unable to support himself on his legs; his face was not so haggard or thin as might be expected, considering the extraordinary state of extenuation of his body and extremities, and in this respect, as well as in general appearance, he considerably resembled the *living skeleton*, lately exhibited in France and in England. He had not the least fever; his digestive organs appeared quite healthy; his breathing natural; and he had no cough, nor did he complain of any pain in the head. To what then was the reduction of flesh and strength owing? Partly to the effects of disease, but chiefly to abuse of those two powerful medicines, mercury and opium. The history of his case may be given in a few words:--He was formerly much subject to gout and gravel; about three years ago he got a chancre and bubo, which yielded to the use of mercury; six months afterwards, in consequence of cold, he was attacked with arthritic inflammation of various large and small joints, combined with mercurial periostitis. The arthritis did not yield to the usual remedies, and he was therefore induced at different times again to try mercury. The constant pain and sleeplessness produced by these complaints rendered him unable to pursue his business, and he sunk into a state of abject poverty. His constitution became more and more impaired, and a cutaneous eruption, in every respect resembling the milder varieties of *rupia prominens*, made its appearance, while an ulcer commencing inside the left nostril, completely destroyed the nasal cartilage, so that the tip of the nose has fallen in. From his account it would appear that some portion of the spongy bones had been also destroyed; one of the spots of periostitis had evidently produced extensive exfoliation of the *os frontis*, but the part is now healed; he has no sore throat; his gums are sound, and his tongue perfectly clean, and moist; he has no thirst, and his appetite is good; bowels quite regular; the few remaining

spots of rupia, the arthritic swellings and pains now become chronic, extreme debility, and an utter want of sleep, except when under the influence of enormous doses of opium, form the catalogue of his present complaints. For the last two years he has never had sleep at night, except in consequence of an opiate; he was first induced to take this medicine in order to relieve his pains, but latterly it is not pain, but the impossibility of sleeping except when under its influence, that has forced him to use it constantly. He has often taken two ounces of Batley's solution in the day! Very large doses of opium act on his bowels as an aperient, and the use of this drug never produces headach, dulness, furred tongue, thirst, nausea, or the least disturbance of the circulating system. For a few nights after his admission into the hospital, he got two drachms of black drop every night, but it was not enough to procure any sleep, and he consequently entreated me to double the dose; but I refused, and ordered the following treatment: three drops of Fowler's arsenical solution three times a day. A nutritious but mild diet; some wine at dinner. Sarsaparilla broth, one pint daily; a starch enema, with one scruple of black drop, three times a day. The good effects of this treatment became soon apparent; his sleep gradually returned, and in the course of a fortnight was sounder and of longer duration than it had been for years. He daily gathered flesh and strength, and, in the course of a month, was so altered for the better, that were it not for the depressed nose, no one could have recognized him to be the being whose misery, a month ago, had so strongly excited our commiseration. The arthritic affection has rapidly subsided, and with returning strength he is regaining the use of his limbs.

The following case exhibits the good effects of opiate injections in a manner not less striking than that just detailed. A professional gentleman of great abilities and strength of mind, about ten years ago was attacked with neuralgia of a very severe description. The disease, which was caused originally by cold, pursued a most anomalous course, giving rise to amaurosis of

one eye, ptosis and permanent strabismus of the affected eye ; contrary to the expectation of both Sir Astley Cooper and Mr. Brodie, to whom he was introduced by his friend the late Dr. Wollaston, the symptoms of cerebral disease made no further progress, but the neuralgic affection of one of his lower extremities became intolerable, occurring in paroxysms of extreme violence, and only to be alleviated by repeated doses of opium. After the lapse of some years, the neuralgia became complicated with pain and swelling of the knee-joint, which still further added to his sufferings, and rendered him a complete cripple. This joint is now permanently enlarged, and within the last two years the lower extremity of the femur seems to have formed an enormous exostosis of an equal growth all round its circumference, but not encroaching on the articulating surface of the bone, which still enjoys the slightest possible degree of motion, although it cannot be moved far from its flexed position. The neuralgic pains, if such they were, have within the last four years been worse than ever. During the paroxysms he has frequently been forced to take 100 grains of opium, much to his annoyance, for he found that it occasioned subsequent nausea and vomiting, stupor, and other unpleasant symptoms, while the constant repetition of this drug had completely destroyed his appetite, and what he most deplored, had sensibly impaired his memory and mental powers. At length he was advised to use it in the form of injection ; the alleviation produced by this change has been most astonishing ; half a drachm of laudanum thus used when necessary, twice or three times a day, effectually alleviates his suffering, and does not produce any of the bad effects before enumerated. His appetite is now good, his spirits cheerful, and his powers of mind unimpaired.

SINGULAR HYSTERICAL AFFECTION.

On the 1st of last September I was called to see a young lady, who was represented to be in a state of imminent danger. On entering the room I found her sitting up in bed, surrounded

by several female friends, all in the greatest alarm. Her face was pale, and her countenance indicated a good deal of anxiety. She held in her right hand a cup containing water, which she applied to her lips about every five seconds, and sipped an extremely small portion of the water, which she immediately swallowed with a considerable effort of deglutition, although the quantity was so trifling; she said that she should be immediately choked if she discontinued this perpetual sipping, and she referred to an intolerable uneasiness at the root of her tongue and in her throat, threatening immediate suffocation the moment she ceased to employ herself in swallowing; and so urgent was the feeling that impelled her to this act, that the moment an attempt was made to take the cup out of her hand, she began to scream with agony, was agitated with convulsions, and to all appearances seemed in the last agony. This scene had lasted for several hours without interruption, and the appearance of the principal actress was rendered still more tragical by a black mass of leeches around her throat, and the blood from their bites trickling down her neck. On examining her more closely I found that there was no obstruction whatsoever to the passage of air through the larynx, and that she could make a full inspiration, without any wheezing or noise in her chest; there was no swelling or redness observable at the root of the tongue, or in the fauces. As the young lady was of an extremely delicate and nervous habit, being very sedentary and subject to frequent attacks of common hysteria, I immediately conjectured that her present symptoms were the result of an hysterical affection, and accordingly I removed the leeches, stopped the bleeding as soon as possible, and gave her draughts consisting of camphor, aromatic spirit of ammonia, and black drop, under the influence of which the nervous irritation soon subsided, and she fell asleep. I have mentioned this case, not because its nature and the proper treatment were not sufficiently evident, or admitted of being mistaken by any practitioner of common attainments, but because it presented some circumstances concerning the act of deglutition

worthy of remark. In the first place, it is clear that the uneasy sensation referred to the throat, was a variety, not an usual variety, however, of globus hystericus. This uneasy sensation was like globus accompanied by the sensation of impending suffocation. The efficacy of the constant sipping and swallowing in alleviating this feeling, may be somewhat analogous to their well known effects in stopping another affection plainly of a spasmodic nature, I mean hiccup, which in most cases may be cured by a similar succession of quickly repeated deglutitions of very small quantities of water ; again, it is worthy of notice, that any attempt to prevent this process, was immediately followed by general hysterical convulsions. How opposite must be the state of the nervous system in hydrophobia, when the slightest attempt to swallow a fluid brings on convulsions.

Another curious affection of the organs of deglutition, has occurred to me in the case of a nervous young clergyman, concerning the state of whose health I was last year consulted, by Surgeon Barker, of Cumberland-street. He complained of various symptoms indicating debility and dyspepsia, but was chiefly annoyed by a painful and convulsive struggle, as he expressed it, which sometimes took place between the bit he had swallowed just before it entered the stomach, and a something that seemed to resist its further passage downwards. This lasted only for a few seconds, and was very distressing both to himself and the spectators, for of course it usually occurred at meals, and rendered him unwilling to dine in society. In another case, these sudden attacks of temporary dysphagia are become so habitual, that the gentleman never ventures to eat, unless a glass of water be within his reach ; for in him the stopping of the descent of the bolus of food, is attended with an urgent sense of suffocation. This gentleman, an excellent anatomist, thinks that the sense of suffocation is entirely nervous, or at least that it has nothing to do with any mechanical obstruction in the glottis arising from the neighbourhood of the descending food. In both these cases, the cause of the disease

appeared to lie in the increased, or rather deranged sensibility of the œsophagus itself. In wounds of the cervical portion of the spinal marrow, it occasionally occurs that the sensibility of the œsophagus is so increased that deglutition is rendered impossible in consequence of pain, a fact sufficient to direct us to apply our therapeutic agents to the neck in such cases as I have related.

In fever I have witnessed several times a very peculiar species of dysphagia, evidently occasioned by flatulent distention of the stomach to such an extent that the lower portion of the œsophagus partook of this condition ; at least, I conjecture so, for during the struggle of the dysphagic paroxysm, a gurgling noise was heard, as if the bit of food was met by a portion of air contained in the lower part of the œsophagus ; my friend, Doctor Autenrieth, of Tübingon, has particularly remarked this symptom, or at least something like it, in what he calls the abdominal typhus fever of young people ; for he says, if the patient takes any drink a peculiar gurgling noise is heard as if the fluid was poured into a lifeless bag. Now, in precisely such a case, Mr. Rumly and I saw a young lady affected, in addition to this noise, with so great spasmodic dysphagia, probably from the entrance of wind into the lower end of the œsophagus, that she altogether refused to drink. This phenomenon gradually disappeared, and the lady ultimately recovered ; but it deserves to be remarked, that in general this symptom and the gurgling noise, described by Dr. Autenrieth, are very bad omens in fever.

GANGRENE OF THE LUNGS, WITH INCIDENTAL REMARKS ON BARRY'S THEORY OF THE CIRCULATION, AND BELL'S THEORY OF THE NERVES.

In 1831, a man named William Deeg, aged 24, died in the clinical ward of Sir Patrick Dun's Hospital, on the 29th day after the first appearance of the eruption of confluent small pox. It is probable that this case would have terminated favourably, had not extensive gangrene of the sacrum taken place, to

which the nurse did not direct my attention, until it was of an alarming extent. It was first pointed out to me on the 18th day, at which time he laboured under hoarseness and bronchitic symptoms, unattended however by any difficulty of respiration. In the course of a few days, however, dyspnœa came on, the wheezing in his chest increased, and seemed to accelerate the period of death, which appeared to all those who had witnessed the progress of the case, to be the result of constitutional prostration, induced by the external gangrene. On dissection, two large and two smaller gangrenous sloughs were detected in the right lung. The gangrenous portions of the pulmonary tissue were insulated, being separated from the surrounding substance of the lung by a whitish membrane apparently formed of coagulated lymph; the question here occurs whether these internal gangrenes were a consequence of the external one, or whether they were the result of the same fatal constitutional derangement that predisposed the external parts to become gangrenous from pressure? The former supposition seems the most probable, at the same time that we must admit that gangrene often takes place in fever in external parts not liable to pressure, as for instance the soles of the feet. It is to be observed, however, that I never knew such parts to become gangrenous, *except after some other portion of the integument had mortified evidently in consequence of pressure.*

Andral's observations in his Clinique Medicale, on the connexion between the state of external and internal parts in fever, and Cruvelhier's Remarks on Gangrene of the Lung, are calculated to illustrate this subject still further, and tend to prove that the gangrenous sloughs in the case before us, were not the result of previous inflammation, although nature had excited inflammation in the surrounding pulmonary tissue, in order to form cysts destined to insulate the gangrenous portions.

In connexion with this subject I may remark that I have seen three cases of intolerably foetid breath, and stinking expectoration, caused not by pulmonary gangrene, but bronchitis.

In all, the sputa were copious, puriform, and evidently bronchitic, and it is very curious that in one man whose body was examined after death, no bad smell was perceptible from any part of the bronchial mucous membrane after it had been cleared of the mucus. The fœtid gas was evidently, therefore, the result of a deranged vital secretion.

In Deeg's case, the pericardial sac was universally adherent to the heart, and yet the circulation was quite natural, an occurrence long ago observed by Morgagni, and which I have also witnessed in several other cases. Bailly, in his *Morbid Anatomy* mentions a case in which the pericardium was altogether wanting. These facts are in themselves sufficient to refute that part of Barry's theory which attaches so much importance to the peculiar mechanism of the pericardial attachment in promoting the circulation. It is rather discreditable to the medical profession, that Barry's theory should have excited so much admiration when first promulgated, as it was formed on principles irreconcilable with well known hydrostatic laws; accordingly ever since his work was published, I have annually in my lectures demonstrated the glaring errors into which he had fallen, and I am extremely glad to find that Doctor Arnott has employed the very arguments I had been in the habit of using, and has given Barry's theory its quietus. With regard to another much more celebrated theory, I mean that of Sir Charles Bell, concerning the nervous system, the analysis of a late writer* makes it proper to state how the case really stands. All physiologists and pathologists are agreed as to the truth of the discovery, not simultaneously but still independently made by Bell and Magendie, that the spinal nerves derive their powers of sensation and motion from different roots, the anterior being destined for motion, the posterior for sensation. The distinction between these roots must be understood with a certain limitation, for late experiments made at Jena, and numerous pathological

* Mr. O'Beirne.

facts prove that the anterior roots are capable of conveying a very slight degree of sensation, and the posterior of exciting a very slight degree of motion. But there is another part of Bell's system peculiar to himself, and upon which he seems to pride himself not a little; indeed he would have just reason to be proud of it were it true. Bell conceived himself justified by his experiments in concluding, that the nervous system, independent of the ganglionic system, and the nerves of the senses, may be divided into, first, nerves of motion; secondly, nerves of sensation; thirdly, a class of nerves which accompany the nerves of motion and sensation, whenever they are distributed to any part possessed of a peculiar or specific power of sensation, or a specific faculty of motion. Thus, according to Bell, the parts concerned in the performance of the respiratory motions are supplied, not merely by common nerves of sensation and motion, but by other nerves destined to associate them together, and insure their unity of action—the common nerves, according to Bell, are the spinal nerves and the fifth pair. This class he calls symmetrical or regular, (original nerves of Shaw.) The other class of nerves he calls unsymmetrical, or irregular, (superadded nerves of Shaw,) and says that they are distinguished by their having but a single root, unprovided with ganglions, and arising from the medulla oblongata and superior portion of the spinal marrow.

The respiratory nerves of Bell, or those destined to associate together the respiratory motions, are the vagus, facialis, accessorius, (spinal accessory,) phrenicus, respiratorius externus, glossopharyngæus, hypoglossus, and the laryngeal branch of the par vagum, (recurrent.) He supposes them to possess this associating power in consequence of their arising from neighbouring and similar portions of the central nervous system. Now, this portion of Bell's theory has been long since refuted by the experiments of Mayow, the reasonings of Alison, the experiments of Eschricht, the celebrated Danish anatomist, and lastly by myself, in my lectures for the last five years. There is an

excellent summary of the arguments against this part of Bell's system in the work of Lund, published at Copenhagen in 1825.*

SMALL-POX AFTER INOCULATION.

It is the duty of the medical profession to maintain the credit of vaccination. The public are impressed with the idea that modified small-pox, (variloid,) cannot happen after the small-pox, but that it is very frequent after vaccination. That small-pox is a better safeguard than the cow-pock, is very improbable, for we now know that the latter is nothing but the small-pox communicated from man to the cow. Besides authors abound with cases of variloid after small-pox itself. The following instance is especially authentic, and I publish it with the permission of Mr. Barker, of Great Britain-street. Mrs. Barker was inoculated with the small-pox in 1801, by Mr. Stewart, then Surgeon General. The inoculation succeeded, and she had the disease favourably. The pustules were no where confluent, and were thinly scattered over her face, extremities, and body. The disease ran its usual course, and was sufficiently severe to leave marks or pits in several places. A few years ago Mrs. Barker was attacked very violently with fever, which after three days terminated in a very copious eruption of variloid; she could trace the infection to a child in small-pox, with which she had been in contact. Mrs. Barker was near her confinement at the time, and in three weeks after the accession of the variloid, was delivered of a dead child, which was covered with the true variolous eruption, at about the stage of pustular maturation. The same poison therefore gave variloid to the mother, and true small-pox to the child. The small-pox communicated to the child in utero is not necessarily fatal. I have known a child born with it, survive. Another remarkable and perfectly authenticated case happened within the circle of my acquaintance:—

* Physiologische resultaten der vivi-sectionen neuerer zeit.

A young lady residing near Dublin, was vaccinated, when an infant. There is reason to believe that the vaccination was not properly watched, so as to ascertain whether it had succeeded. When eight years old this young lady caught the true small-pox from infection, the disease was not at any period of its progress dangerous, and left no pits. At eighteen years of age, she was again attacked, but the disease was now variloid, and not attended by secondary fever. She did not escape however so well with regard to its consequences, for she was evidently marked. In such cases as has been long ago remarked, the pits on the surface of the skin are very different from those left by the pustules of true small-pox.

ART. XI.—*Case of Enormous Steatomatous Tumour.* By JOHN COLVAN, M. D., copied from notes taken while Acting Surgeon to the Armagh Infirmary.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICINE AND CHEMICAL SCIENCE.

Armagh, February 15, 1833.

“SIR,

“If you consider the following case worthy of notice, you will please insert it in a corner of our *National Journal*, and oblige a subscriber and a zealous friend and admirer.

“JOHN COLVAN.”

JOSEPH NICHOLSON, æt. 44, admitted Thursday, July 17, 1823, a married man, tall and thin, of dark complexion, and apparently leucophlegmatic temperament, one of the society of friends, lives 5 or 6 miles from Armagh. The tumour has been 12 years in arriving at its present very great size, it commenced like a pea, or small wart, and at first increased but slowly; lat-

terly, however, it has increased very rapidly, and although *it never gave any pain*, yet its great weight and unwieldy bulk cause considerable annoyance, and his appetite and strength are much impaired. It also completely prevents his following his usual employments. The apex of the tumour, or part by which it is attached, measures a foot, and its base measures two feet in circumference. It may be stated as commencing about two inches above the right shoulder joint, and rather in front of it, and extending down the front of the chest and arm for several inches; it is quite moveable, but about the middle of its attachment, appears to have rather deep connexions. It has very little feeling, as a pin thrust nearly an inch into it, *produces no pain*. The veins on its surface appear exceedingly large and tortuous. It feels uniformly very firm in its texture, and in some places even *as hard as bone*. He is very willing to have it removed, and anxious that I should do it, as some others to whom he had applied deterred him from the operation. Doctor John Johnston, R. N.,* has been kind enough to assist on the occasion, also Messrs. Leslie and Reild, pupils of mine at the Infirmary. I purpose making a double elliptical incision, including a portion of the integuments, and endeavouring, after the removal of the tumour, to bring the edges of the wound together by adhesive plaster, or sutures, or both. I commenced my first incision, the patient having been properly placed, on the pectoral side of the tumour, beginning at its upper part, and carrying it down with one sweeping cut to its base; I then made a similar incision on the humeral side, both incisions meeting in a point above and below, including the portion of tegument I intended removing as superfluous in covering the after wound. In prosecuting the dissection of my first incision, near the edge of the axilla, I cut a vein of *immense magnitude*, which lay so deeply covered, as to prevent its being easily seen, or secured. The gush of blood, in a stream as thick nearly as

* An esteemed friend, since dead.

a walking cane, was instantaneous and tremendous; the poor man fainted, and was nearly falling off his chair; he was placed in a horizontal posture on the floor, while I secured by pressure, with all my force and ingenuity, the orifices of the great blood-vessel. After much difficulty and many trials, I succeeded in passing a ligature round each of its extremities, and thus prevented further effusion. By cordials, &c. we restored him again to his senses, when I represented to him what had occurred, and asked him if he was willing that I should proceed? He said, "I leave it all to yourself, do what you think right." Here a little murmur occurred among the by-standers, whether I should proceed. It instantly occurred to me, that I could not carry on the operation, in the usual manner, as another such occurrence would inevitably kill the man; I had no other idea, however, but to proceed, and most fortunately a method presented itself to my mind, that enabled me to do so, with comparatively little risk to my patient. I proposed to my friend, Dr. Johnston, whose kind support and ability were of essential service, to arm two aneurism needles, and to prosecute my dissection of the attachment of the tumour most cautiously; where a large vein would occur, to cut carefully around it, and having tied it in two places, to divide it in the centre, and thus to save any further hæmorrhage of consequence. After a moment's consideration, and having the needles armed, I commenced the most anxious, arduous, and difficult piece of dissection imaginable; obliged to act, and yet half afraid, lest at every step I should sever one of those enormous veins, I proceeded amidst all this difficulty, now at one side, and again at the other, still watching and calculating where my enemies lay, when I met one, detaching all around it, and tying it as above, and then dividing it. The operation was in this way considerably protracted, and I believe occupied about 20 or 25 minutes, but was concluded with safety to my patient, as scarcely any blood was lost after I adopted my latter method of operating. The number of arteries requiring the ligature were only three, and these

no larger than the radial ; the number of veins, 10 or 12, and these of amazing size ; some of their open mouths capable of containing a finger. The lesser ones of course I did not tie, as they required no ligature. I brought the wound together with adhesive plaster, and a considerable part of it healed by the first intention, the ligatures came away in good time, the granulating process was a little tedious, but he left the Infirmary completely cured, in about four weeks, having the perfect use of his arm and hand. I have seen him several times since, he is stout and well, and much more lusty than he formerly was, in consequence of the great quantity of blood that was drained off to this large tumour, now supplying the general circulation.

Observations.—It is worthy of remark, how few, if any, nerves these large tumours have. A woman presented herself to me, not long since, with one that she thinks of having removed, it is as large as a child's head, of three or four years old, it is between her shoulders, and I thrust a pin nearly one inch into it without her *feeling it at all!* and she says it is often torn and abraded by accident, and she never feels it, nor would she know, but by seeing the blood. The size and number of the arteries also, being so small in proportion to the veins is curious ; the veins too, being distributed in large trunks, vessels of small size being rarely met with in the dissection. Nicholson's tumour was purely sarcomatous, the fatty matter impacted in cells with such firmness as to give the feel of bone externally, it weighed $9\frac{1}{2}$ lbs. avoirdupois, and was in size and shape not unlike a cow's udder.

ART. XII.—On the
Joint. By
General to the
to the King

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Mr. Crampton's

1st. It would appear, asserts, that before a muscle in a state of preparation must be produced either the muscle along the axis acting directly for the great tendency to enlargement; a comparison of the

a recently dislocated spasmotic contracture and not from the ruptured supported by not be major and danger to enlargement; a comparison of the

IN the first of
two forms
most frequently

I. The dislocation *above*,
humerus is lodged in the axilla, and

II. The dislocation *forwards*,† in which the
humerus is lodged, immediately below the clavicle, and under
the pectoral muscle.

I shall now, with a view of connecting the pathology with the practice, briefly advert to the practical inferences which may be drawn from the facts which the anatomy discloses.

Those inferences, it will at once be seen, are, with one or two important exceptions, in accordance with the practice of the most experienced surgeons, ancient as well as modern, they have, therefore, but slight pretensions to originality, but they are not, perhaps, on that account the less worthy of attention. It is always of great importance to be able to add the sanction of reason to the conclusions of experience; without this sanction, the best rules of practice are liable to a misapplication, which may put them on a par with the worst; besides, rules of practice, which rest *merely* on what is called *experience*, can have but slight claims to our confidence, when we remember that the *experience* of one age, or of one country, is usually found in opposition to that of another.

* Dublin Journal of Medical and Chemical Science, No. vii.

† Called *inwards* (en dedans) by the French Surgeons.

1st. It would appear that the resistance to the reduction of a recently dislocated shoulder proceeds altogether from the spasmodic contraction of the irritated muscles about the joint, and not from “the neck of the bone being tightly embraced by the ruptured capsular ligament.” The latter opinion, though supported by some of the highest authorities in surgery,* cannot be maintained against the evidence of pathology; the painful and dangerous practice of violently twisting the joint, “in order to enlarge the rent in the capsular ligament,”† should therefore be abandoned. If muscular contraction be the only, or at least the chief obstacle to be overcome, it would follow that previously to applying extension, we should have recourse to such means as have a tendency to diminish the tone of the muscular system generally, and to exhaust the irritability or power of contraction in the muscles of the affected limb in particular. To effect the first object, we may employ general bleeding, the warm bath, and nauseating doses of tartarized antimony; and for the second, nothing is so effectual as gentle, but *long continued* extension. But there is a property connected with muscular power, of which we may often avail ourselves with great advantage, even previously to having recourse to means which are calculated to act *directly* in diminishing the force of muscular contraction. It has been justly observed, by John Hunter, that “muscles may be taken by surprise,” and their force in this way

* Petit, Duverney, Heister, Desault, White, Thompson, &c.

† “J’ai dit en qu’en quelques rencontres, l’ouverture de la capsule, trop étroite pour laisser rentrer la tête de l’humerus étoit un des obstacles principaux à la réduction. “*Agrandir le passage en déchirant de nouveau les bords que la forme ;*” telle est alors la manifeste indication qui se présente ici on la remplit en faisant exécuter à l’os des grands mouvements, sort de circondution sort de rotation sur son axe en le portant brusquement en différens sens.”—Desault, vol. i. p. 371. This practice, it is to be observed, is recommended in *recent* dislocation, but upon a totally different principle from that in which it is afterwards recommended by the same author in old dislocations, where the design is to destroy the adhesions which result from inflammation.

eluded rather than overcome ; he asserts, that before a muscle can put forth its full force, it must be in a state of preparation for action, and this state of preparation must be produced either by the stimulus of the will conveyed to the muscle along the nerves, or by a mechanical or chemical stimulus acting directly on the muscle itself. He accounted in this way for the great resistance which a joint opposes to an external force tending to produce dislocation, when it is *prepared* to meet it, as compared to that which it opposes to a force which takes it *unawares* ; a man, for example, may leap from a height of 20 or 30 feet, and land on the hard ground, without producing dislocation of the ankle, yet he may suffer this injury by merely slipping *unexpectedly* from a height of one foot, as when (miscalculating the number of steps in a flight of stairs) he steps down one more than he reckoned on. This law of muscular action is now universally acknowledged, and may be applied with advantage in the reduction of dislocations.

If before assistants are called in, or any apparatus is applied, the surgeon, while he appears to be occupied merely in ascertaining the nature of the injury, applies a gentle extension at the wrist, and slowly raising the arm to nearly a horizontal position, suddenly pulls it upwards and a little forwards, (that is towards the patient's face,) while at the same time he as suddenly pushes the trunk backwards by pressing, with the left hand, below the axilla, he will in a great number of *recent cases* succeed by this simple process in reducing the dislocation. His success, however, will in a great measure depend on the *unexpectedness* of the attempt ; he should, therefore, endeavour to divert the patient's attention from his proceedings, and I know of no means so effectual for this purpose, as inducing him to describe circumstantially every thing connected with the occurrence of the accident ; this is a theme on which all patients, who are at all able to express themselves, are sure to expatiate with the greatest satisfaction, and once engaged on so engrossing a topic, it will require but a small degree of tact on the part of the Sur-

geon to seize the favourable moment when he can apply his force with the greatest advantage.*

II. It would seem that in *luxation into the axilla*, muscular contraction operates in opposing reduction by pressing the head of the humerus (particularly that part of it in which there is a fossa separating the proper articular surface from the greater tubercle) against the inferior part of the brim of the glenoid cavity, and it would appear from a case described by Sir Astley Cooper, that the muscle which is chiefly if not exclusively concerned in causing this pressure is the supra-spinatus,† the obvious practical inference is to relax this muscle, as well as the deltoid, by raising the arm to nearly a right angle with the body, previously to applying any extending force, and above all things, not to use *any* force in pressing the head of the bone *upwards*, for so long as it lies on the cervix of the scapula, the pressure upwards only tends to lock it more firmly with that bone, and when it is once clear of the brim of the glenoid cavity it will start into the socket by the mere traction of the muscles.

III. With respect to the *direction* in which the extension is made, for the reduction of dislocation into the axilla, the practice of experienced surgeons, in all countries, seems now to be nearly uniform; the arm is usually raised to nearly a right angle with the body, and while a gentle extension is made by pulling at the wrist, the arm, used as a lever, is moved upwards and downwards, and to each side, so as alternately to relax all the muscles about the joint. It is true that in the method of reduction by the heel in the axilla, (which has the reputation of being a

* It would be uncandid not to state, that this combination of mental and manual dexterity is much more generally and I believe successfully applied in France, than in these countries: this will be readily allowed by all those who have had the good fortune to witness the amusing and instructive scenes, which sometimes take place on the occasion of reducing dislocations in the Hotel Dieu.

† I next divided the deltoid muscles, and then found that the supra-spinatus muscle was my great opponent.—*Treatise on Dislocations*, p. 377.

very successful one,) the arm is drawn downwards in a direction nearly parallel with the body; but it by no means follows, because this method is often successful, that the force which is employed is most advantageously applied; the desideratum is to effect reduction, with the least possible degree of violence to the parts, and if it were ascertained by comparative experiments, conducted on a large scale, that two methods of reduction were equally successful, but that the object was effected in one by the employment of half the force which was employed on the other, it is manifest to which the preference should be given. And again, if it be true that in some cases the supra-spinatus muscle, retaining its connexion with the greater tubercle, is the cause of the resistance to the extending power,* it is obvious, that this resistance can be best overcome by raising the arm, and thus relaxing the opposing muscle. The success which not unfrequently attends the method of reduction (first recommended by White of Manchester) by drawing the arm directly *upwards*, in a line parallel to the axis of the trunk, is no doubt to be attributed chiefly to the relaxation which it effects of the supra-spinatus and deltoid muscles; it is probable, also, that in this position of the humerus, the head of the bone is in some measure unlocked from the neck of the scapula, against which it is (when dislocated downwards) strongly compressed by the contraction of the muscles. Several years ago the method of White was frequently resorted to in this country, for the reduction of old dislocations of the humerus, (as the large ring in the cross beam of the anti-room in Steevens' Hospital testifies;) the method, however, has of late years fallen into disuse, a proof that it has no great advantage over the means now generally employed.† The British surgeon will be a little surprised to find this method introduced, as "a new and important mode of reduction," (see *Lancet*, No. 485,) in the *Hôtel Dieu*. M. Mal-

* Sir A. Cooper.—*Treatise on Dislocations*, p. 377.

† Mr. Hey of Leeds strongly recommends the method of White, for the reduction of old dislocations, particularly when the head of the bone lies under the pectoral muscle.—*Hey's Observations*, 1810.

gaigne, the gentleman who introduced the practice to the notice of M. Dupuytren, stated in his address to the class, that the anatomy and pathology of this kind of dislocation had led him to adopt the method before he was acquainted with *the method of Mothe*; the method of Mothe, as stated by M. Malgaigne, "consists in making extension, the arm being lifted up forcibly and consequently shortened, instead of extending the limb in a depressed and elongated position." M. Mothe's memoir, in which he lays claim to the *invention* of this new method of reduction, was presented to the Academy of Surgery in 1785. Mr. White's paper, in which he particularly describes the same mode of reduction, was printed 1764.

IV. In Dublin, as in Paris, and in Berlin, and in Germany generally, we apply our extending power to the fore-arm in preference to the arm; the application is far less painful, as any person can ascertain by applying alternately strong pressure to his own fore-arm at the wrist, and to the arm above the condyles of the humerus;* besides we think that the pressure above the elbow causes the biceps and triceps muscles to contract more forcibly; and lastly, when the extension is made at the wrist, the surgeon has more power in directing the motions of the limb.

V. Great stress is laid by most surgeons on the advantage of fixing the scapula, as it is called; it may be doubted, however, whether the thing be possible, or if possible, advantageous. It is quite plain that a split cloth, or a napkin with a hole through which the arm is passed, can, when the arm is strongly extended, act only on the inferior costa of the scapula, or rather on the walls of the axilla formed by the edges of the latissimus dorsi, teres major, and pectoralis major muscles; the whole effect of this force can be no other than to push the inferior angle of the scapula backwards and upwards, consequently

* There is scarcely a part of the body so intolerant of pressure as the inside of the arm immediately above the condyles, this is no doubt owing to the superficial situation of the ulnar nerve and its proximity to the bone at that spot,

to direct its superior angle, and the glenoid cavity downwards, and, by acting on the pectoralis major and latissimus dorsi, to draw the head of the humerus inwards towards the ribs, that is, to remove it *from* the glenoid cavity. To obviate this objection, some surgeons recommend pressure to be made by the hand of an assistant on the acromion of the scapula, so as to push it backwards while the humerus is drawn downwards and outwards; but it is plain that unless the force which the surgeon applies to the head of the scapula to keep it back, be at least equal to the extending force which is applied to the arm, the scapula cannot be *fixed*, it *must* follow the arm; besides when the arm is raised, the deltoid fills up the sub-acromial space and renders it impossible to apply any appreciable force to the acromion. As the neck of the scapula cannot be pushed *upwards*, it is proposed by Bonn,* to disengage the bones by pressing the head of the humerus *downwards*,† at the moment when the extension is at the utmost; the proposal is a most rational one, and has been adopted for several years past in the County of Dublin Infirmary, as I think, with considerable advantage.

VI. When a greater power of extension is required than can conveniently be applied by the unassisted strength of men, I have for upwards of twenty years employed the mechanical power of the lever, in preference to the pulley. The lever has in the first place the advantage of being always at hand, as there are few places in which a pole or a ladder cannot be readily procured; but its superiority over the pulley consists in the facility with which the direction of the extending force can be varied, while the force is still maintained, and the suddenness with which the force can be withdrawn as soon as it has produced its effect. The manner in which I am accustomed to employ the ladder, as a lever, will be best understood by a reference to the subjoined sketch.

* Commentatio de Humero Luxato.

† In the direction E, see Plate II. fig. 3, in No. vii. of this Journal.

VII. The anatomy of the recent case of dislocation *forwards*, (see Plate I.) settles the long disputed question as to whether or not the humerus can be dislocated *primitively* in any other direction than *downwards*, or into the axilla.* It is quite plain that in the case of Wilson, the head of the bone was thrown at once forwards into the situation in which it appears under the clavicle, (see Plate I.) as the inferior portion of the capsular ligament was not ruptured, and the attachment of the subscapularis and teres minor muscles, to the inferior costa of the scapula remained undisturbed. Perhaps, there is not in the whole compass of surgical pathology, any point, the determination of which is of more importance in a practical point of view than this. For if (acting on the opinion so generally entertained, that in dislocation forwards, the head of the humerus *first* escapes into the axilla) we endeavour to draw the humerus downwards, it is obvious that the attempt must be attended with the most extensive injury to the subscapularis muscle; in fact, the muscle must either be torn right across, or separated from its attachments to the scapula or to the humerus.

The objection to drawing the head of the bone *downwards* into the axilla, for the reduction of dislocation *forwards*, of

* Among the questions to be solved by future experience, Baron Dupuytren proposes the following. "Can the humerus be dislocated *primitively* only in one direction, and are the other luxations only *consecutive* on this? If it be true that there exists but one primitive luxation, in what direction does it take place? The Baron's own opinion is, that primitive luxation *forwards* may take place, but he considers it to be "of very rare occurrence, and generally consecutive to luxation into the axilla."—See Lancet, No. 485. Sir Astley Cooper, on the contrary, "does not believe in any change of place after dislocation, when the muscles have once contracted." "The bone (he adds) is generally thrown at once into the situation which it afterwards occupies, so that excepting from circumstances of great violence, the nature and direction of the dislocation are not subsequently changed."—*Treatise on Dislocations*, p. 372. Primitive dislocation forwards, is unquestionably rather a rare occurrence, yet in by far the greatest number of *old* dislocations, we find the head of the bone under the pectoral muscle; it seems probable, therefore, that dislocation *forwards* may exist either as a *primitive* or *consecutive* affection.



Fig. 1. Aspidistra

Young, from the seed, and a young plant from the seed

The specimen of Aspidistra in Fig. 1. is from the seed

FOLD

OUT

course, applies exclusively to the primitive dislocation in that direction. In such a case the clear indication is to force the head of the bone *backwards* towards the glenoid cavity, the axis of which is as nearly as possible in a line with that of the head of the humerus; this can be effectually done by applying a fulcrum immediately below the axilla, and using the dislocated arm as a lever of the first kind; the surgeon should therefore place his left arm extended horizontally, immediately below the walls of the axilla, between the dislocated arm and the chest, and then grasping the wrist in his right hand, he should draw the arm forcibly across the patient's body. This view of the subject did not escape the sagacity of Ambrose Parey, and when we consider the age in which he lived,* it is impossible not to admire the boldness with which he puts aside the authority of Hippocrates, (an authority at that time more supreme in medicine, than that of the Pope in religion), when it was found in opposition to his personal experience.†

CASE.—The Hon. Col. Gore of Dublin Castle, aged about 56, a healthy and vigorous man, was overturned in his carriage on a very dark night, the carriage falling sideways from a causeway, which was raised six or eight feet above the level of the adjoining field. When the side of the carriage struck the ground, he was conscious of having received a severe shock on the elbow, which rendered the arm powerless; he sustained no other injury. I saw the Colonel in less than an hour after his accident, and found that he had suffered a dislocation of the left humerus forwards. Standing before him, I placed my left arm extended horizontally under his axilla, and grasping the wrist in my right

* The middle of the 16th century.

† "Hippocrates, shut up within the strait limits of the lesser Asia, never saw this kind of dislocation," (forwards). He then gives the case of "a certain nun, who weary of her nunnery, cast herself down out of a window, and bore the fall and weight of her body on the elbow, so that her shoulder was dislocated forwards."—*Parey's Works*, p. 394, London, 1640.

hand, I drew his arm rapidly across his body, so as to bring the hand in contact with the right hip ; the bone snapped into the socket at the first effort.

EXPLANATION OF PLATE III.

FIG. 1.

The manner of employing the ladder for the reduction of a dislocated shoulder.

A. The foot of an assistant placed on the lower end of the ladder, to prevent its rising when the upper end is pressed down.

B. A folded sheet or table-cloth for the counter-extension ; the ends may be made fast to a ring, or other fixture in the wall, or may be held by two or three assistants standing on a table.

C. The hands of the assistant, who slowly depresses the lever, until the surgeon who stands at D. (astride of the ladder), announces that the bone has returned to its socket. While the extension is in progress, the surgeon from time to time presses the upper part of the humerus *downwards*, to disengage it from the brim of the glenoid cavity ; once disengaged, it springs upwards into the socket, without the assistance of pressure from below. The surgeon, by standing astride over the ladder, has it in his power by the pressure of his knee on either side, to give a lateral motion to the lever, while the extending force is still in operation. In dislocation *forwards*, the surgeon should press his left knee against the side of the ladder ; this motion, by bringing the hand forwards, will throw the head of the humerus backwards.

FIG. 2.

A convenient mode of applying extension at the wrist.

The knot is made by passing the ends of the ligature twice through the loop. The patient's hand is to be passed through the middle part of the tie, (*a*), the ends when drawn tight are to be brought parallel to each other, (the one on the ulnar, the

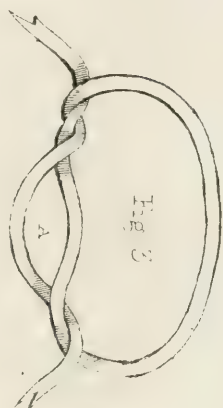


Fig 2



other on the radial side of the hand), and made fast to the round of the ladder. A folded silk handkerchief forms the best ligature.

ART. XIII.—*Observations in Dispensary Practice.* By
CHARLES PATTERSON, M.D., Physician to the Rathkeale
District Hospital, and to the Rathkeale Dispensary.

ASPHYXIA OF NEW-BORN CHILDREN.

WHEN a child is still-born, it is presented to us under circumstances similar to those, in which we find a person, who is asphyxiated from exposure to irrespirable gases. In the former, the blood being deprived of the placental regeneration, and the lungs failing to assume their office, a fluid, which is incapable of supporting the nervous energy, circulates through the brain, and life becomes extinct; just as in the latter, when in consequence of the absence of oxygen, the nervous system becoming inundated with non-aerated blood, vitality is abolished. This similarity, between the still-born and asphyxiated, applies more particularly to that asphyxia, which arises from immersion in carbonic acid gas. In this, the blood simply ceasing to be arterialized, all the functions are instantly suspended; and though the asphyxiated person should be removed into air, favourable for respiration, yet, for the renewal of that process, the respective organs must be excited into action. So in the new-born infant, though generally the respiratory apparatus is excited into action, by the ordinary influences to which the child is exposed at birth, yet, a variety of circumstances may determine the inefficacy of these influences, to accomplish the first act of independent life; when the placental circulation being interrupted, the blood must cease to be arterialized, and the child will die still-born, unless more powerful means of functional excitement be applied.

It being hence obvious, that in both cases death arises from similar causes, and is preceded by a similar negation of excitability; it is evident, that that agent, which has been found most powerfully to produce resuscitation from asphyxia, should be expected to act, no less energetically, in rousing into all the activity of life, the respiratory organs of a stillborn child. Impressed with this view, which must have repeatedly occurred to every person, and having often had occasion to regret the unsuccessful issue of the most assiduous observance of those directions, which are laid down in the works of obstetric writers, and taught in the schools, for the treatment of still-born children, even in instances where the infant exhibited symptoms of foetal life, immediately subsequent to birth, I conceived, that in a case where the ordinary means had been unavailingly employed, I should be warranted in having recourse to cold affusion, that wonder-working agent in asphyxia from non-aerated blood.

In two instances of still-born children, I have had occasion to make use of cold affusion, and in both, with the most satisfactory effect.

CASE I.—Mary O'Brien of Rathkeale, aged 24, in June 1831 was seized with premature labour of her first child. She stated, she had only reached the commencement of the eighth month of gestation, and attributed her impending abortion to fright. Her pains being slow, after about thirty hours of lingering labour, she was delivered of a still male infant. Immediately, on the protrusion of the foetus, a very languid circulation was perceptible in the funis, and a slight motion of the limbs was observed. Previous to the division of the cord, it was attempted to excite respiration; but the funicular pulse having quickly ceased, the child was removed and subjected to artificial inflation, friction, external warmth, and nasal irritation. There being no appearance of benefit, and ten minutes having been lost in these fruitless attempts, I placed the infant in a tub, and twice dashed over it three quarts of water, the temperature of which was about sixty degrees. On the first dash, a

slight convulsive motion of the body was sensibly excited ; after the second, the heart and lungs were in evident action, but this was exceedingly weak and tremulous. While the babe was allowed to remain for a few moments in the water, which scarcely reached its ears, the thoracic parietes were subjected to strong friction. In effecting this, the integuments were made to glide, to and fro, over the ribs, so as to excite titillation. Movements of the arms and legs, and active respiration having quickly succeeded, the child was removed from the vessel, well dried, and wrapt up in flannel. It slowly acquired strength and activity, but ultimately, became remarkable for its large size and healthy appearance.

CASE II.—The second case was also that of a male child, and occurred soon after the other. The mother, Ellen Tierney, aged 30, after a tedious labour of about three days' continuance, was delivered of her first child still-born. At birth, the head of the child having been expelled, the pains failed for several minutes, but being reinduced on the administration of the ergot of rye, the body was rapidly ejected. Neither on the expulsion of the head, nor on the final delivery, did any symptom of lingering life present itself: yet, from the appearance of the infant, vitality could not have been long extinct, and the hope was entertained that resuscitation might be effected. I therefore lost no time in separating the funicular connexion, and immediately had recourse to cold affusion. This acted promptly and successfully. At first, there was a momentary shuddering, then an almost imperceptible heaving of the chest was observed, and in the next instant, the heart's action was comparatively vigorous.

Both these children are yet living, and afford ample proof of the safety of the practice, if not of the preference, which on further experience, cold affusion may be found to claim over the usual means of restoration. In these instances, I forbore to make use of the warm bath, from a conviction of its being here a very injurious preliminary to cold dashing, and from the re-

collection of a case of a still-born child, reported in one of the late periodicals, wherein it was manifestly attended with much disadvantage.

HEMERALOPIA.

Hemeralopia, or that form of periodical blindness, wherein the patient can see very well by day, but very indistinctly at night, either in moon, or candle light, is an affection which most persons have described as rarely originating in these islands. Mr. Lawrence goes so far as to say, that he never saw a case, which was not an importation from some intertropical climate. It would seem, however, that the disease is not so very infrequent, at least in this part of Ireland, as might be supposed from the statements of medical writers. The affection is here often met with in dispensary practice. Yet there are connected with the district, no circumstances which could lead one to infer, that its inhabitants, more than their neighbours, are subjected to the operation of that cause, to which the complaint has been generally attributed; that is, a too dazzling direct or reflected light of the sun, whose vivid rays are said to act on the retina with powerful and injurious effect. It has rather appeared within my observation, that the constitution of the individual was in fault, that the disease has arisen from visceral derangement, and not from solar or other external mischievous influence.

Hence, I am inclined to question the correctness of the general conclusion, that, because the disorder is frequently met with in warm latitudes, therefore, it for the most part arises from the paralyzing agency of a tropical sun.

It may not be urged, that the temporary weakness of vision, so generally experienced on passing on a moonlight night, from a well-lighted room into the open air, is illustrative of the origin of hemeralopia, on the principle that the latter arises from a somewhat similar transition, after the patient's exposure to the dazzling effect of an intense solar light. For the tempo-

rary weakness of sight, from over excitement in the former case, and hemeralopia, are very different affections. The latter is a periodical blindness, which has its invasion at night-fall, runs its course, and disappears before morning. Though the times of its recurrence are apparently governed by the sun's revolution, yet it is totally independent of the presence or absence of light; for the hemeralopia is equally dark, whether in the vivid splendour of a flambeau, or in the dusk of twilight.

The following cases are examples of the disease having had a purely constitutional origin. In each, hepatic obstruction formed the first morbid link in the chain of phenomena; then jaundice followed, in connexion with which hemeralopia supervened. This was easily removed by remedies calculated to restore the healthy action of the liver, without any attention to the local ophthalmic affection. If then hepatic derangement in this country be capable of producing the disease, it is rational to suppose, that it is equally efficient in tropical regions, where the general prevalence of liver obstructions would readily account for the frequent observance of night-blindness.

There are districts in France, in Poland, and in Russia, where hemeralopia prevails to a considerable extent. But it is said, in these places local peculiarities powerfully promote the reflection of the solar rays. Perhaps, on inquiry, it may be hereafter discovered, that the great liability of these districts, arises from the aliment and habits of the people, who from the circumstances of these, are more subject than their neighbours to visceral disorders.

CASE III.—James Dwyer, aged 19 years, applied at the Rathkeale Infirmary, the 9th September, 1831. He complained that every evening at dusk, he became almost blind, and continued so till sun-rise next morning; that he could see very imperfectly by fire or candle light, and that exposure to heat and active exercise, or labour, always considerably aggravated his inability to discern objects at night. He stated, that in candle light, however vivid, he could distinguish no object that was

directly opposite to him. He could see a little in an oblique direction, and this enabled him, with much difficulty, to grope his way. The pupils were permanently and very much dilated. "He felt stupid and confused in his head." There was great fulness, hardness, and some tenderness of the right hypochondrium; conjunctiva yellowish; alvine evacuations white. Early in May 1831, he was attacked with jaundice. He did not get rid of it for a month, and while labouring under the disease, he first noticed the derangement of sight. This continued with only daily intermissions, up to the period of his application.

Having been bled, blistered, and purged, with little effect, he was directed to rub in one drachm of strong mercurial ointment every night, and as soon as this had affected the constitution, recourse was had to the nitro-muriatic acid bath. The moment the gums became tender, he experienced a manifest improvement of his sight. But at this time he complained, for a few days, that exposure to artificial light produced most unpleasant sensations in his eyes. Under the treatment, the hepatic disorder was quickly and effectually relieved, and with it the amaurotic affection entirely disappeared.

CASE IV.—In November, 1831, Patrick Mangan, aged 32, applied for relief, and stated, that "about six weeks before, he was affected with jaundice, which continued for eight or ten days, but having been badly cured it fell on his eyes. Ever since, he is unable to see any object at night, however bright it may be, neither can he see in a lighted room." There was pain and fulness of the right hypochondrium; dilated pupils; yellow tinge of the conjunctiva; and high coloured urine with pink sediment.

He rubbed in ungti. hyd. fort. ʒi . every night, used the nitro-muriatic acid bath, and took an occasional dose of calomel and cathartic extract at night, followed by infusion of senna and sulph. magnesiae in the morning. Through these means his amendment was rapid. In ten days he was able to read the

smallest print by candle light, and became entirely free from hemeralopia.

CASE V.—The 14th December, 1831, James Wynn, aged 18 years, became a patient at the Rathkeale Infirmary. He complained of languor and loss of appetite. Skin and conjunctiva yellow; alvine evacuations white; urine high coloured, resembling beer; no pain of side or epigastrium on pressure. He first noticed the yellowness of skin three weeks before his application, and from that period he stated, he was regularly attacked with almost perfect blindness at dusk every evening. It continued the whole night, and disabled him from going about, unless led by some person. His sight was natural in the day time. Objects did not even appear of a yellowish hue, as they have been said to do to jaundiced patients. The pupils were very much dilated; bowels costive.

His bowels were briskly moved every morning for five or six days, by *pulv. jal. comp. and calomel*, with the effect of diminishing the jaundiced state of the skin, but without the least benefit to his nocturnal loss of vision. Recourse was then had to the constitutional effects of mercury, and the system having been brought under the influence of that mineral, he completely recovered his usual acuteness of sight, in discerning objects at night.

In the present year, I have also met with other cases of hemeralopia, but they were much slighter in degree, than those which have been related. The subjects were generally males. Two were at the time labouring under slight jaundice of a few days' continuance. The others had lately recovered from a similar affection. In all these cases, the *nitro-muriatic acid bath* was employed; and as it was found effective in producing permanent cures, no attempt was made to induce mercurial constitutional action.

Since my attention has been directed to the connexion, which appears to exist between hemeralopia and jaundice, I have observed in every case of the latter complaint, that has

fallen under my observation, more or less tendency to the former affection. In all, there existed preternatural and fixed dilatation of the pupils. And when questions were put concerning the state of vision, in most instances complaints occurred, in some degree, of its nocturnal impairment.

CARIES OF THE VERTEBRÆ.

Spinal disease of that form wherein the bodies of the vertebræ, or their cartilages, are affected with caries, is certainly one of the most deplorable affections, to which the youthful of both sexes are liable. To avert its ravages on the constitution, and stay that deformity which almost ever attends it, are objects that have long been regarded, as only attainable by the painful and troublesome process, first recommended to public notice by Pott. But, as every medical practitioner must have felt the inconveniences and irksomeness of issues, it cannot be doubted that in the treatment of this most lamentable complaint, every other remedy which could present the least hope of success, must have been duly and repeatedly submitted to trial. Hence, it is reasonable to suppose, that in combating this malady, so clearly associated with a scrofulous origin, application must long since have been had, by various physicians, to the specific powers of iodine. Therefore, the following cases are merely reported, as interesting instances of the efficiency of that powerfully sanative agent, in different forms of spinal disease.

CASE VI.—In August, 1831, on my appointment to the Askeaton dispensary, among the patients then in attendance on that institution, I found James Moran suffering under extensive disease of the dorsal vertebræ. The three middle bones seemed to be principally engaged, and considerable mischief had already ensued. This patient had just attained his fourteenth year. His countenance, pale and withered, presented the appearance of much greater age. He was unable to walk, and on attempting to stand, he could only attain the semi-erect position; in which, grasping his thighs to support a tottering body,

this unfortunate creature reached little more than 42 inches in height. His body and extremities were excessively attenuated. There existed troublesome diarrhœa, alternating with profuse night sweats. But above all, he complained of excruciating pain of the back, which without any remission, rendered his days miserable, and his nights sleepless. The pain absorbed all his feelings, and was so agonizing, even to his friends, that its alleviation was regarded as the utmost extent of their hope.

On examination, a greatly projecting curvature of the spine presented itself in the situation already referred to. The body of the sixth vertebra seemed to be totally destroyed, the fifth and seventh had in a great measure shared the same fate, and the anterior portions of the vertebral bodies bounding the chasm having approximated, a hump-back deformity was produced, the magnitude of which may be easily imagined. On each side of the protuberance there was an issue, and around these the integuments were considerably swollen and inflamed. Nine months had elapsed since the insertion of the peas, and yet, the miserable boy had experienced not the least diminution of those evils, with which he was so grievously afflicted.

I prescribed for him quinine, cicuta, and hyosciamus, in combination, three times a day, with an additional dose of the narcotics at night. He was brought again to the dispensary on the 24th of August, and as there was no reason to conceive that any improvement had taken place, recourse was then had to the administration of iodine; this, in the form of tincture, was administered in five drop doses every eight hours. He was reported on the 7th December to have obtained most decided relief. The pain of back considerably easier; diarrhœa and night sweats greatly diminished; refreshing and natural sleep, to which he had long been a stranger, were encouraging circumstances, that induced a vigorous perseverance in the use of the ioduretted tincture. Under its agency, the improvement of this poor patient continued progressive, and his mother reported on the 17th of October, that all pain and uncasiness, save what the

issues occasioned, had entirely subsided. She urged, that the issues were never attended with benefit, and having anxiously requested permission to discontinue them, it was allowed.

This unfortunate boy was again brought in to the institution, and I saw him a third time, the 31st of October. The vast extent of amendment caused me considerable surprise; his countenance had regained the plumpness and complexion of health; the limbs had acquired rotundity and strength; he walked firmly without assistance; had experienced no pain in any part, since the removal of the peas; diarrhœa and night sweats had totally ceased; sleep undisturbed; appetite craving, and digestion good. All the functions were perfect and natural, except respiration, which from the altered form of the thorax seemed to be slightly oppressed. And of all the evils under which he had suffered, the deformity of the spine alone remained, an encumbrance for life.

The boy did not again attend at the dispensary, but I have lately heard, that having had no return of his former complaints, he continues in the enjoyment of excellent health.

CASE VII.—Mary Gibbons, aged 26 years, became a patient at the Rathkeale Infirmary, the 12th of November, 1831. She principally complained of pain across the loins, and of an abscess which was discharging in the right groin. The pain, she attributed to kidney gravel, and stated, that she was unable to retain her urine for any convenient time. On inquiry, I learned, that she had been ailing for several months, with severe pain in the situation of the lower dorsal, and upper lumbar vertebræ; that she complained of numbness and weakness of the inferior extremities, which felt heavy and unmanageable, and that she experienced considerable difficulty in walking. It was two months since the abscess appeared like a small tumour, and gradually becoming larger, it burst, as she stated, about ten days previous to her application. The discharge was profuse, and with a bad appetite, and irregular hectic symptoms, she had altogether a very unhealthy appearance.

On examining the spine, a slight projection of the spinous processes of the last dorsal and first and second lumbar vertebrae was observed, but it did not amount to curvature. Pressure over these bones produced an excessive degree of pain in the part, and was followed for several minutes by shooting pains down the right thigh.

This patient commenced taking five drops of the tincture of iodine three times a day, the dose was at different periods increased to ten drops, which she never exceeded. Under the exhibition of this remedy, she improved with remarkable rapidity ; increase of appetite, healthy change in the countenance, abatement of local pain, and improvement of the general health, were the more immediate appearances of amendment ; then gradual cessation of the inguinal discharge, and restoration of the functional powers of the urinary organs and lower extremities. Within three months her recovery was perfected, and no perceptible vestige of disease remaining, she discontinued the medicine.

CASE VIII.—(From private practice). In July, 1832, having been requested to visit Miss ———, a young lady, about fourteen years of age, and of delicate appearance ; I was informed, that about four or five years before, she had suffered from severe pain in the back, attended with loss of power in the lower extremities, and that, for these complaints, an issue was inserted on each side of the vertebral column, at a part where morbid sensibility presented itself. The issues were kept open to the end of twelve months, when the young lady and her friends, conceiving that she was perfectly cured, suffered the sores to heal up ; the medical attendant having acquiesced in that measure. From that time nothing of moment occurred respecting this young lady's health, until last April, when she again began to experience pain in that part of her back, where she had been previously affected. This pain was referred to the lower dorsal region ; it had been gradually becoming more troublesome and unremitting ; her health seemed to be declining ; appetite

bad ; and there existed considerable mental depression. The depression was principally owing to the recollection of her former sufferings, which, from the similarity of circumstances, she dreaded that she should have again to experience. Subsequently to the recurrence of pain, the lower extremities became considerably affected ; she suffered from numbness, and shooting pains in the thighs ; and at the period of my visit, complained of great inability to govern the action of her feet ; she frequently tripped ; could not get up stairs without laying hold of the balustrade ; and in consequence of striking her toes against the steps, had repeatedly fallen. On examining the back, I found the spinous processes of the four lower dorsal vertebræ, projecting about the fourth of an inch beyond their fellows. The projection, unlike curvature from destruction of the vertebral bodies, seemed as if produced by the intervertebral cartilages and bones being elongated from before backwards, in consequence of softening and compression of their substance. Pressure over these spines occasioned a great deal of pain, which was also felt shooting round in the direction of the left hypochondrium. The pain was more intense in the situation of the eleventh and twelfth dorsal vertebræ, and it would seem, that these had now, for the first time, become the seat of disease ; for the cicatrices of the former issues were situated opposite the ninth and tenth dorsal spines ; but these also were exceedingly painful when subjected to pressure. Pressure did not affect any other portion of the vertebral column.

This case was treated with tartar emetic ointment applied on each side of the spine, and tincture of iodine administered in five drop doses every eight hours. In three days a pretty numerous crop of ecthymatous pustules supervened to the tartar emetic inunction, and run its course within a week with considerable irritation. When Miss ——— had been about three weeks engaged in the use of the ioduretted tincture, she reported herself exempt from all pain, either of the back or extremities ; and the firmest pressure over the vertebræ

failed to elicit the least morbid sensibility. At this period, a rather profuse epistaxis having occurred, I judged it advisable that the employment of the iodine should be suspended. The hæmorrhage ceased, but in four or five days, the vertebral pain returned with almost its original intensity. As soon as there was reason to suppose that the hemorrhagic disposition had subsided, the medicine was resumed with the most decisive effect; the pains and morbid sensibility, as well of the back as of the extremities, almost immediately disappeared; the countenance became plump and ruddy, the appetite and digestion good; and having acquired a perfect command over the lower limbs, she now, in November, 1832, walks erect with firmness and ease. On the whole, the health and strength of this young lady, with a buoyancy of spirit and of action, have reached a degree of excellence from which they had long been estranged.

None of these persons observed the horizontal position; the two first being dispensary patients, it was needless, in opposition to the prejudices and feelings of themselves and their friends, to attempt to enforce its observance. In the last case, from the fretful despondency of Miss —— and her delicacy of constitution, it appeared exceedingly questionable, whether the probable benefit from position, where the disease was situated so low in the spine, would be even equivalent to what she should necessarily suffer from restraint and confinement.

These are the only cases of spinal disease, in which I have yet had an opportunity of putting the extraordinary powers of this medicine to trial; but from its unequivocal and successful agency in these instances, I have little doubt, that for this malady, so dreadful in its effects, iodine will be found a remedy no less efficacious than it has been for bronchocele.

ART. XIV.—*The Sequel of the Case of Pneumothorax, described in the Third Number of this Journal.* By JAMES HOUGHTON, M. D., Physician to the South Eastern General Infirmary, &c.

THE case of pneumothorax and empyema of the left side, published in this Journal last July, was recorded as an instance of the extensive ravages which disease can make on the human economy, even on the organs nearest concerned in the vital actions, without destroying life, and also as a singular example of an apparent attempt at the spontaneous cure of this disease, under circumstances which seemed to cut off the possibility of such a result. The interest which attached to it on the latter score is much diminished by its termination, yet as its history serves to exhibit the same resistance to the extinction of life, which marked it in its progress as before detailed, I am induced to give a short account of it here in continuation. This appears to be the more called for, as death has occurred, and extinguished any distant hope which might at one time have been entertained, that this case would prove an exception, and by extraordinary physiological vigour of the system, triumph over our pathological conditions, which in every other instance on record, had produced a result more or less rapidly fatal. Moreover, the observation of its course, combined with the details of the dissection, has been attended with much instruction, and may be acceptable to the readers of the former account.

I strongly recommended M. J. to give over his labour as a bricklayer, and go to some relations who lived in the country, telling him, that as his constitution had shewn such vigour in making so unexpected a rally, it was not impossible, that if he would but take favourable means he might ultimately recover. He was, however, exactly of the opposite temperament of that which anticipates death, for notwithstanding the frequent declaration to him of my opinion, that he must not expect any other

termination to his malady ; I invariably found him, the next day, or if I saw him again the same day, talking quite confidently of hopes of recovery. After my last recorded report, he continued at work without much change for some weeks, but at the end of the day he was very much exhausted, particularly by acute pains about the sacrum, which increased his dyspnœa while they lasted. He got cold from working himself into a heat, and became affected by a copious hæmoptysis. This continued for some days, but being abated by medicine, it could only induce him to cease from work for one day. All this time there was scarcely any alteration in the physical signs. The right lung retained the same muco-crepitus above the mamma, and on the left side the proportion of air and fluid remained stationary. The tintement métallique became very obscure, although never imperceptible. It appeared as if the hæmoptysis came from the cavity, which was indicated at the top of the compressed lung, as an increased gargouillement was manifested.

Working about the middle of July in damp ground, his feet got wet. This produced a decided impression on him. He was seized with pain in the right side, his dyspnœa increased, a racking cough supervened with expectoration, and together with this affection of his remaining lung, epigastric tenderness and constant pain in the right iliac region, thirst, anorexy, loaded red tipped tongue, with accelerated pulse, betokened a gastro-enteritis attack. He continued working with these symptoms daily increasing, until at length on the 21st July, the dyspnœa became so overwhelming, and the pain in the right side so violent, that he was fairly forced to give up, being unable to stand any longer. I had been out of town for a few days previous, so that he applied for admission into the Meath Hospital, where he was taken in by Dr. Stokes.

On admission. The pain of right side was extremely acute ; the respiration high and hurried ; the right side expands to a great extent during inspiration ; dullness on percussion from mamma downwards ; feeble respiration over same space, puerile

in superior part. There is no ægophony, but a decided frottement heard; the integuments of this side tender; tension and soreness in epigastrium; face anxious; thirst; pain in iliac region, and vomiting. He was cupped on the right side and bled, and got pills of calomel and opium. This treatment removed the very urgent symptoms. The pain remained however in the ilio-cæcal region, but was ultimately relieved by repeated applications of leeches, and other remedies, as was also the diarrhœa. The affection of right lung remitted considerably, but recurred with violence on the subsidence of the abdominal symptoms. A blister was applied over the side, by which the inflammation was manifestly abated. This is a condensed statement of his illness during his stay in the Hospital, taken from copious notes kindly given me by Dr. Stokes. He regarded it as an attack of external pleuropneumony of the right lung, with inflammation of the gastro-enteric mucous membrane. The last note of the Hospital book relates, that the right lung was nearly free, but the muco-crepitus which formerly occupied a part of the mammary region still persisted. The physical signs proper to the left side remained unaltered, for the latter appeared to be quite unengaged in this inflammatory attack.

He left the Meath Hospital in a weak state, from the effects of this severe inflammation of his remaining lung and intestinal canal. He refused to remain till his convalescence would be further advanced, and the day after his dismissal, he was actually engaged in some brickwork, which was then going on at the Hospital. Instead of more closely following him, I insert a note taken on the 19th October. He has been working for two months, he says his strength is increasing, his tongue is very red at tip, and the remainder overlaid with a doughy paste, which is slate coloured, along the middle line, his bowels are occasionally too free, but no constant diarrhœa, he eats his meals with appetite, severe pains in the sacral region obstruct him in stooping, and give him great torture at work, he has been wishing for a fit of hæmoptysis, which relieved those pains be-

fore, but none has occurred. He has an occasional short ringing cough of the same character as before the attack of pleuropneumony of the right side. He lies now equally well on both sides, and on back. The fluid is heard on succussion, but not nearly so plainly as formerly. It is ascertained by percussion to extend upwards, to about the space between the fourth and fifth rib. The tintement is audible on coughing, speaking, and even on breathing. At the superior part of the lung a feeble cavernous respiration is heard, with a single bubble of gargouillement. The right lung sounds well on percussion. The same muco crepitus remains above the mamma, loud puerile respiration around the latter, and in the other parts of this side.

About this time he went to live at the other side of the city, so that I saw him very unfrequently for the next two months. During this period he got an attack of bronchitis from exposure during work, which greatly increased his dyspnœa, and exhausted his strength.

On the 26th December, he called on me in such a deplorable state of dyspnœal exhaustion, that he was scarcely able to speak. He told me that for a fortnight he had become affected with a severe pain in the left (the pneumothorax) side, and that he was obliged, notwithstanding, to lie on it, to be able to breathe; the pain at right side was so violent, that he could not refrain from shouting. He had an intense bronchitis of right lung, evidenced by loud sonoro-mucous rale, which in some parts became crepitus. The diarrhœa had returned for some time, and become constant, by which he said he was greatly weakened. On applying the stethoscope to the pneumothorax side, while the dyspnœa was increased by the exercise of the car on which he came, the amphoric resonance was uncommonly loud, air bubbles were heard bursting through the fistula in strings, making a sound, like blowing with a pipe into shallow water, contained in a large bottle. The same was heard posteriorly at the angle of the scapula very plainly, and more or less

of the tintement could be heard all over this side, from the fifth rib upwards.

On the 27th December, he went into the Meath Hospital again, where under appropriate treatment, both the thoracic and abdominal symptoms were temporarily mitigated. The relief was however but trifling, the diarrhoea continued. For this, he got pills of nitrate of silver and opium, which restrained it, but as it became checked, the thoracic symptoms gained ground, and he at last sunk exhausted under the accumulation of organic lesions, from which he suffered. He died 25th January.

The body was opened the next morning by Mr. Hudson, and inspected by Dr. Graves, Dr. Stokes, and several gentlemen at the Hospital, and I was assisted in a closer examination of the lung, by Mr. Houston, to whom I gave it as a preparation for the College of Surgeons' museum.

The chest being measured, the left side was found to be one inch and a quarter less than the right. The mediastinum was thickened. On opening left side, air escaped slowly, and the cavity was perceived. About three pints of sero-purulent matter, in which were suspended flocks of lymph, occupied about one third of the cavity, the remainder of which was filled with air, both fluid and air were inodorous. The sac occupied the whole length of the left side, from the summit of the lung to the diaphragm. It was formed of a dense fibro-albuminous structure, about a line and a half in thickness on average, and was so intimately attached to the costal and pulmonary pleura, that no separation could be found; at the places of reflexion, at the summit and diaphragm, it was much thicker than elsewhere. Its inner surface was red, and in many places coated with a flocculent exudation of lymph, evidently the result of a recent inflammation. The lung lay flattened in its whole length against the mediastinum and spine, scarcely filling a third of left side of thorax. On blowing into trachea with a bellows, air bubbled out of three fistulae, situated in the same line on the level of the third rib. The anterior one, situated opposite to the junction

of the cartilage and rib was the largest, nearly the size of a shilling, another posteriorly about half this size, and the third between these two. The lung was somewhat carnified. At its summit was an anfractuous cavity, capable of containing the smallest sized hen's egg. This was separated from the dense fibrous false membrane covering the summit, only by a thin film of lung, two small cavities were near this large one, and tubercles of various stages were scattered through the lung, particularly at its superior part; there remained, however, much intervening tissue which was in the stage of pneumonic congestion, occurring in a carnified lung, as well as some spots of hepatization occurring under the same condition. At the base of the lung, however, was a solid oval mass of grey induration, two inches in its long diameter, which contrasted strikingly with the rest of the lung affected with pneumonia. In this spot it had evidently taken place before the lung became compressed, and carnified by the pleuritic effusion, as its solid granular structure preserved the appearance of the condensed air vesicles, which in other parts of this compressed lung were nowhere to be found. A bloody tenacious mucus filled the bronchial tubes, the lining of which was deep red.

The large anterior fistula presented to the first view the appearance of a cul de sac, the flat base of which was lined by a smooth membrane. It resembled somewhat a strongly marked fossa ovalis of the heart, seen from the right auricle; on its face were five little holes about a line in depth, which could not be followed by a probe, they looked very like the orifices of bronchial tubes which had been closed up. Under the superior part of the rim of the apparent cul de sac, a probe passed easily into a tolerably large bronchial tube, which led to the cavity at the top of the lung. Each of the other fistulæ were traced through bronchial tubes into minute vomicæ, one of which was filled with a cheesy tubercular matter. At the lower part of the empyematous sac covering the lung, were two holes which perforated the thickness of the false membrane, but did not commu-

nicate with the lung. They were either small ulcerations of the sac tending to become fistulæ, or the remains of fistulæ which had been blocked up by the inflammation of the sac.

The cavity at the summit of the lung was lined by a very thin membrane, more of the serous than the mucous character, quite dissimilar to the membranes which usually line cavities of so long a standing. There was no trace, except at the superior part, of the semicartilaginous layer, which lies next the pulmonary tissue in phthisical cavities, but the lining of the small vomicæ of tubercular matter, presented their ordinary characters.

The right lung adhered firmly to the costal pleura, by a very thick and strong organized false membrane, which covered the whole of it. It contained a few congregated tubercular masses, in the upper and middle lobes; the great mass of it was in the state of pneumonic congestion, which here and there had advanced to the stage of hepatization; besides this, there were many patches of intervening sound tissue to be met with. An appearance well worthy of remark existed in the interlobular fissure. A space of the latter about two inches square, did not participate in the adhesive process which firmly agglutinated the rest, and as the edges were covered by the dense false membranes, which invested the whole of the pulmonary pleura, this space resembled exactly the cavity of a pneumonic abscess, and might easily have passed for one if examined with little attention.

The heart lay exactly in the median line of the body, it was large, firm, and sound in structure, and perfectly free from morbid alterations within.

Small ulcerations were visible at the end of the ileum. The cæcum was extensively ulcerated, over a large space of it the submucous tissue was quite bared, and studded with numerous tubercles; small islets of mucous membrane remained here and there on the ulcerated surface.

It is to be regretted that the brain was not examined, but it

may be remarked, that his craniological development was particularly good.

With regard to this dissection, one of the most interesting points for consideration which it furnishes, is the origin of the fistulæ. Many circumstances favour the view which would be *a priori* entertained, that they were produced by the bursting of tubercular vomicæ into the pleura; this appears supported by their occurring near the top of the lung, where there was a cavity and some tubercles. Yet their actual examination did not demonstrate this so unquestionably as might be expected. No remains of tubercular matter were found in immediate contact with their orifices; we had to traverse bronchial tubes for some space, before we found evident connexion with vomicæ, which last were certainly not those by which the pleura was bursted, being, as I have stated, at some distance from it. What made me entertain doubts as to the manner of the formation of the fistulæ, was the observation of the ulcerative erosions which are to be seen towards the lower part of the false membrane covering the lung. They strongly suggested the probability, that some of the fistulæ at least, were formed by the ulceration spreading from the pleura to the lung. And, although it may not be deemed sound reasoning, to give a distinct origin to different fistulæ in the same lung, I am inclined to imagine, that the anterior one alone was caused by the bursting of a small tubercular cavity, which caused the pneumothorax and empyema, and that the two narrower posterior ones were produced by ulcerations, originating in the false membranes, (in the same way as those which are to be seen below), which penetrated the lung, and thus found a communication with the bronchial tubes leading to the cavity. Whether this be true with respect to these two fistulæ, the erosions alluded to are important pathological appearances.

Another interesting point for observation, was, the parietes of the cavity at the top of the lung, their state was quite dissimilar to that which is usually met with in cavities of so long

standing as this was. Instead of the double membrane which lines each, (the soft pulpy one towards the cavity, resting on a semicartilaginous membrane, in contact with the pulmonary tissue), here the cavity was clothed by a thin layer like serous membrane, studded with granular particles of lymph, apparently of the same recent origin, as the dense flocculent shreds which were generally diffused over the empyematous sac. These last were evidently to be referred to the last period of his case, about six weeks before his death, when the intense pleuritic pain, and other symptoms, betokened an inflammation of the sac.

It is to be remarked, that this patient from the time that the pneumothorax occurred, was scarcely at all affected by hectic, although tubercles existed in the lungs; and the history of the case shows, that he was affected with it before the time marked as the most probable for the occurrence of the fistulous communication. This fact is more worthy of observation, when considered in connexion with the peculiarity of the lining of the cavity, which, as just mentioned, no longer retained the features which are always met with in cavities, where the patient sinks under phthisis. Broussais and Andral remark, that the hectic of empyema is not accompanied with profuse night sweats, unless there be tubercles in the lungs, or membranes of the pleura.* In this case the hectic ceased from the time of the empyema's commencement, and it seems warranted to conclude, that the progress of the tubercular development was simultaneously checked. It has been remarked, that where pneumothorax occurred, in a patient who was in the last stage of phthisis, from tubercular cavities in *one* lung, if the patient survived the immediate consequences, that life was rather prolonged than shortened; and it was suggested to me, by Mr. Hudson, a zealous medical student, that this event was connected with the diminution of the hectic, which has been also observed to ensue. It is an interesting subject for investigation, how far these consequences depend on putting the compressed tuberculated lung,

* Cyclopædia of Medicine, article Empyema, by Dr. H.

hors de combat, into a state of quietude. It has often occurred to me, reflecting on the inevitable progress of tubercles deposited in the pulmonary tissue through the stages of softening, and abscess with their fatal concomitant hectic, this progress which is so much more rapid and certain here than in other organs, was much promoted by the constant motion which is necessarily more or less kept up in them, and about them, by the respiratory actions of the lung. The greater vascularity of the pulmonary tissue is, doubtless, another condition which conspires with the motion to the same effect. This is an important pathological consideration, which cannot be followed up here, but its application to the case of pneumothorax may be pointed out. The lung here becomes compressed, and incapable of performing its functions. The consequence is, that the side does not dilate, and the pulmonary tissue undergoes little or no motion. The blood too is sent in such diminished quantity, through the compressed lung for arterialization, that it becomes in a comparative state of anemia. Thus two conditions are produced, which would, according to this view, be peculiarly favourable to arrest the progress of the tubercles.

M. J., since he was seen first by me, a year ago, has laboured under empyema and pneumothorax of the left side, and in addition to this, has had a few tubercular developments in the intestines; in July he became affected with an intense pleuropneumony of the whole of the right lung, and inflammation of the cæcum; in October with a general bronchitis, and gastric symptoms; and finally, in his last illness, January of this year, with intense bronchitis of both sides, and partial pneumonia, with an acute inflammation of the false membrane, forming the sac of the sero-purulent effusion, and with an extensive ulcerative inflammation of the large intestine. All these lesions were demonstrated by the post mortem inspection.

Could this man have recovered? However this may be, his case has been found an exception to all others of a similar nature, in its event. But, notwithstanding the impression which

the progress of his case nine months ago, made on my mind, has not been erased by his death, I am still of opinion, that an attempt at cure was being made, and this opinion is shared by others, better judges than me, who also witnessed the case. We know now that several instances have been observed, where tubercular cavities have cicatrized, and the patients recovered. The case of empyema by absorption, is a fact, which is also well authenticated. The experiments of Nysten and Speip have shewn that air is even speedily removed, by absorption from the pleural cavity. In this case, there was decided evidence of a partial absorption of the contents of the sac, and, moreover, the anatomical examination of the cavity, and of the oldest fistula, shewed alterations which may be attributed to the same curative effort. We have before noticed the appearance of the lung of the cavity, to which allusion is made; in the fistula this was evinced by the smooth well-organized appearance of the serous membrane, which formed its base, and again, by the minute holes having all the appearance of obliterated bronchial tubes. Now, could he by any means have been led to see the necessity of abstaining from work, of going to the country, avoiding exposure to pulmonary inflammations, in short, making voluntary efforts to co-operate with nature in the promotion of his cure, instead of thwarting it by all his habits; it seems not totally inconsistent with reason, or even with experience, to have expected, that a constitution which made a rally under such unfavourable circumstances, and so long resisted the organic lesions detailed above, (many of which, it must be recollected, he brought on by his own imprudence), might have improved its partial attempt to a perfect recovery.

ART. XV.—*Remarks on the Composition of the Iodide of Platinum.* By ROBERT J. KANE, M.R.I.A., Professor of Chemistry to Apothecaries' Hall, &c.

THE difference between the results of Lassaigne and myself as to the composition of the iodide of platinum being such as could not be accounted for by any possible error in the analyses, immediately on the receipt of his memoir, I recommenced the investigation of the subject, and I am glad to find that it has since engaged the attention of Mr. Phillips, who arrived at the conclusion I had myself adopted, that where a difference existed between my results and those of Lassaigne, the error was most probably on my side, and such I have since determined by experiment to be the case.

Having lying by me a quantity of the iodide that had been prepared at the time of the former experiment, I analyzed it, and found that it gave the results before obtained, viz., $1\frac{1}{2}$ atoms of iodine + 1 atom of platinum. It was, therefore, evident that the source of error was not in the analysis.

Having prepared some perchloride of platinum, freed well from proto-chloride, it was decomposed by hydriodate of potash, and the precipitated iodide collected. In this decomposition iodine was separated, but in small quantity. The iodide of platinum so prepared possessed all the properties by which it has been characterized by Lassaigne and myself. Two analyses were made of it.

In the first, 60 grains were heated to redness; there remained behind 17.7 of platinum.

In the second, 60 grains were heated to redness; there remained 17.4 of platinum.

This iodide was, therefore, by these analyses, composed of—

	Iodine.	Platinum.
1st. - -	42.3	17.7
2nd. - -	42.6	17.4
	—	—

On Lassaigue's supposition it should be—

Platinum	-	-	-	-	16.8
Iodine	-	-	-	-	43.2

On my former view—

Platinum	-	-	-	-	20.55
Iodine	-	-	-	-	39.45

These analyses prove evidently that Lassaigue is correct, and that probably in consequence of using a perchloride, holding in combination some proto-chloride of platinum, I obtained a mixture of the two iodides of platinum, which of course gave, on analysis, a result intermediate to them.

In reply to a letter from me to my friend Chevalier, Lassaigue stated that having made the iodide of platinum so far back as the year 1829, and determined its composition, he is of course entitled to be considered its discoverer ; but he did not at that time develope its properties ; he had not recognized its power of uniting with the basic iodides ; and in his answer (an abstract of which and of my letter appeared in the *Journal de Chimie Medicale*, for January 1833), he at once allows me priority in that portion of our respective researches, and states his satisfaction at our results agreeing so closely. It will be seen, on reference to my paper, that so far from wishing to deprive him of the honour he so well merits, I expressed my regret at not being able to find his original paper, (my series of the *Journal de Chimie Medicale* commencing 1830, the year that the *Société de Chimie Medicale* did me the honour to elect me a corresponding member,) and extracted from Berzelius' *Traité* all that illustrious chemist says of the substance ; and as he did not mention its composition, I naturally concluded that it was not known. To Lassaigue is, therefore, due the discovery of the two iodides of platinum, but as Lassaigue has allowed me priority in forming its various saline combinations, I trust that no philosophic knight errant shall deem it necessary to contradict what the actual experimenter has approved of.

In his observations on my paper (*Philosophical Magazine*, March, 1833), Mr. Phillips states that he objects to my nomenclature, and wishes that I would recur to simpler views. I am sorry that he has postponed his remarks on my mode of viewing these combinations, as I have long wished that some of these men whose opinions we habitually reverence, they being the scientific leaders in these countries, would discuss the validity of the doctrines advanced within these few years by Berzelius, Bonsdorf, &c. Of these there are none better qualified for the task than Mr. Phillips; and as I care not at what loss of self-esteem I attain to a knowledge of the truth, the refutation of me would serve as a *coup d'essai* to prove his arms before engaging the more illustrious men of Abo and of Upsal.

BIBLIOGRAPHIC NOTICES.

Notices concerning Works on the Practice of Physic, Surgery, Pathology, and Physiology, recently published in Germany.

IN the following *Notices*, it is intended to give a succinct view of some few of the manifold improvements in medical science, that are produced almost daily by German industry and research. The difficulty of procuring German books, and more particularly valuable monographs of a very recent date, must necessarily render these notices extremely imperfect, and incomplete ; still, however, it is hoped that they may prove acceptable to the profession, and may stimulate some person more fortunately situated to undertake the same task on a much more perfect and extensive scale, and in a manner more worthy of British Periodical Literature.

It is to be observed that the following notices claim the indulgence of the readers, on account of the detached and insulated manner in which they are given. Much more copious materials than we have at hand would be necessary to enable us to present a connected and arranged view of the improvements lately made in each particular department of medical science. Such a task would indeed be more suited to a society, than an individual, and its accomplishment in this country, we may hope for at no distant period.

I.—*Tractatus Anatomico-pathologicus sistens duas Observationes rarissimas de Formatione Fibrarum Muscularium in Pericardio, atque in Pleura Obviarum, &c.* Georgius Leo-Wolf.—Heidelbergiæ et Lipsiæ, 1832.

THE facts communicated in this inaugural thesis are of an extremely rare and interesting character, and their accuracy is vouched for by the Professors of Medicine in the University of Heidelberg, and is still more satisfactorily attested by preparations of the morbid appearances referred to. Five quarto

plates beautifully executed in lithography, accompanying the treatise.

The woman, whose heart presented the very remarkable appearance hereafter to be described, died at the age of 56, and it is observed, that her sister had likewise fallen a victim to disease of the vascular system, viz., aneurism of the abdominal aorta; and that her father had been affected with extensive ossification of the thoracic aorta; her health, with the exception of occasional attacks of rheumatic headach, and arthritic pains, had been very good until some years before her death, when she became subject to a perpetual feeling of anxiety, often accompanied by palpitations of the heart, and sudden fits of debility, preceded by transitory glows of heat. The origin of these symptoms she attributed to a fright occasioned by her clothes catching fire.* During the last year of her life, she was much affected by pains chiefly in the left side of the body, and finally, by symptoms of dropsy, hydro-thorax, and derangement of the heart's action. On dissection, a large quantity of water was found in the chest, and none whatsoever in the abdominal cavity, an occurrence, as our author observes, by no means usual.

“That wonderful heart (*cor illud mirabile*), proceeds our author, before the sac of the pericardium was opened, appeared natural in its size and position, but on cutting the pericardial sac, both the ventricles and auricles seemed somewhat smaller than natural. The external or fibrous layer of the pericardium was normal in its texture, whilst the serous layer by which it is lined internally, appeared much thicker than usual, and had altogether lost its natural transparency. That portion of the serous layer which is reflected over, and embraces the heart itself, was neither thickened, nor otherwise altered in structure, and as usual, adhered intimately to the subjacent substance of the ventricles, which were found degenerated into a fatty mass. The external surface of this reflected serous layer, was covered with *striae* and *floculi* of coagulable lymph, superimposed in a laminated manner, but easily removable, so as to expose the serous layer covering the substance of the ventricles just spoken of.

“Between these two serous layers, viz., that lining the fibrous sac of the pericardium, and that reflected over the substance of the heart, was found a stratum of perfectly muscular substance (*massa omnino muscularis*). The extent of this muscular stratum answered exactly to that of the reflected serous layer of the pericardium, and, consequently, it covered the whole body of the heart, ventricles, as

* In a lecture lately published in the London Medical and Surgical Journal, I have related several cases of organic diseases of the heart and stomach, produced by violent frights; in one of my cases *fire* was likewise the cause of the fright.

well auricles extending from the great vessels issuing from the base of the heart to its very apex. In estimating the relative actions of the muscular fibres composing this layer, it is necessary to recollect that on the different sides of the heart they ran in different directions, imitating in the most perfect manner the natural spiral course of the muscular fibers of the heart, and forming towards its apex a junction by means of a *vortex* of muscular fibres. When examined by a powerful microscope, these fibres were found to be composed of separate fasciculi, each again containing distinct minute bundles or parcels of still smaller fibres, just as is observed in true muscle. The muscular fibres were strong and powerful on the posterior face of the left ventricle, but still more so where they embraced the origin of the inferior vena cava, like a sphincter muscle."

On the whole, our author observes, that this remarkable adventitious stratum of muscular fibres coincided very nearly in its arrangement and distribution with the most external layer of muscular fibres observable on the surface of the ventricles in a heart of the usual form. This layer, too, was perfectly muscular and organized, where it looked towards the cavity of the pericardium; but it may be readily understood, that its more deeply situated fibres were less completely organized, and finally they appeared to form a confused mass incapable of distinction or analysis, where it lay in contact with and adhered to the striæ and flocculi of coagulable lymph already spoken of, and which formed the bond of union between this muscular stratum and the serous layer of the pericardium, which embraces the heart itself. Nothing very remarkable was observed in the form or structure of the auricles, but the structure of the ventricles had undergone a very singular change, its external portions being no longer muscular, but converted into a fatty mass.

The extent to which this degeneration had proceeded is most accurately detailed by our author; for our purposes it is sufficient to remark, that it no where had affected the parietes of the ventricles quite through their whole thickness, and, consequently, it every where left a stratum of muscular substance between itself and the ventricular cavities. This remnant of the muscular substance of the heart varied much in thickness, in the left ventricle not exceeding one line, while in various parts of the right ventricle it was two lines thick. Its fibres seemed quite distinct in colour, consistence, and chemical composition from the fatty layer which covered it, and from which it was every where separated by an abrupt line, except in some few spots, where the muscular fibres could be traced for a short distance within the fatty substance. This muscular stratum was further extremely remarkable for the looseness, tenuity, and want of

strength and firmness in the fibres of which it was composed. In fact, they were in that state of softening accurately described by Laennec,* as “attended by a striking loss of colour, so as to resemble the palest dead leaf,” a state which subsequent observations have shewn to be frequently combined with fatty degeneration of the remaining muscular substance of the heart.

This softening was not so well marked in the left as in the right ventricle, and was still less observable in the interventricular septum. All the valves, as well as the columnæ carneæ, appeared to be healthy. After the anatomical details of this curious case, the author relates the experiments made by Gmelin, the author of the celebrated treatise on digestion, on the chemical composition of the fibrous stratum above spoken of, from which it appears that its chief component principle was fibrin.

The next case related by our author is scarcely less worthy of attention than the first. A man, 43 years old, of robust habit, was suddenly attacked with acute pleuritis;† after several relapses, the disease became chronic, and he died in a year, after severe suffering. A large quantity of coagulable lymph was found in the right side of the chest, and the costal pleura appeared much thickened and opaque, in consequence of being covered by organized pseudo membranes, and in several parts was minutely injected, exhibiting beautiful vascular ramifications derived from the intercostal arteries, and distributed in a reticulated form. The chief branches of these vessels were found not merely supplying the pleura costalis, but likewise perforating this membrane in their passage inwards. Our author then proceeds to the chief point of interest in the case. A strong expansion of muscular fibres lined the internal surface of the costal and diaphragmatic pleura. This muscular layer consisted of fibres, of the red colour peculiar to muscles, and was so evidently muscular in all its physical qualities and organization, that it was thought quite unnecessary to submit it to the test of chemical analysis. Where this muscular stratum passed over the ribs, it was most closely and intimately adherent by means of dense cellular membrane to the pleura costalis that covered these bones. These muscular fibres arose from the intercostal spaces of the second and third ribs, and at their origin

* Forbes' Translation, p. 608.

† Concerning the cause of this pleurisy, he thus expresses himself: “*Eamque metastaticam, et a scabie celeriter suppressâ ortam esse, luce clarius fuit.*” This savours strongly of the doctrine propagated so extensively in Germany by the writings of Autenrieth of Tübingen, but which is now abandoned, particularly in the northern schools.

were minute and delicate in their structure. After passing over the third rib, their appearance rapidly changed, and increasing in number and strength, they soon attained to the size of thick fasciculi,* connected by means of lateral fibres, ramified in such a manner as to form a muscular expansion spread over the whole internal surface of the costal pleura, and extending to that of the diaphragm. The vascular network before spoken of was most remarkable and copious towards the bottom of the thorax; and here also it was observed that the muscular stratum was thickest and strongest. The costal pleura covering the first and second ribs exhibited no trace of muscular fibre, but was covered by coagulated lymph of a yellow rather than a reddish colour. Where the muscular fibres passed across the ribs, their course, arrangement, and form was somewhat altered, so that they adhered more intimately together, and also to the subjacent bone by means of minute tendinous fibres. As a consequence of this arrangement, the muscular stratum, where it crossed each rib, presented some resemblance in structure and appearance to the tendinous intersections of the recti abdominis muscles. Being in these places copiously mixed with tendinous fibres, their structure was condensed and firm, but immediately on reaching the margin of each rib, the muscular fibres regained their former preponderance, and between the ribs simulated the appearance of intercostal muscles. These pseudo-intercostal muscles were of considerable breadth, on account of the great dilatation the intercostal spaces had undergone from the pressure of the pleuritic effusion. The muscular fibres which over the ribs pursued a straight course parallel one to the other, assumed a very different arrangement when they reached the diaphragmatic pleura, for they then became united together in bundles, and no longer distributed in the same plane, they formed bands in some parts, radiating, as it were, from a common centre, and in others convoluted and twisted among each other in such a manner as to include spaces of various shapes, but generally bounded by waved and curved outlines. This structure appeared in all that portion of the adventitious muscle in contact with the pleura where it lines the muscular portion of the diaphragm, as well as where it covers its central aponeurosis. When more minutely examined, it was easy to see that its reticulated form corresponded in the latter position to the fibres of the tendinous portion of the diaphragm, and that so accurately as to represent to a certain extent the manner in which they are interwoven. This, our author ob-

* Fortes densosque oculis exhibuerunt fasciculos.

serves, displays the sagacity of Nature in a very remarkable manner, as the arrangement of these muscles and their relations with regard to the parts with which they were in connexion was evidently most wisely adapted to the proper performance of the function they were intended to perform. To conclude, the lung on that side of the chest was carnified in consequence of the long continued pressure of the puriform fluid.

After giving an account of the morbid phenomena observed in these two remarkable cases, our author proceeds to reason upon the manner in which the adventitious muscles were formed, and the *modus operandi* pursued by nature in producing such curious results. In the first place, he argues, and I think correctly, that serous membranes may themselves be the seat of inflammation, and that then they are every where traversed by vessels containing red blood, and giving rise to peculiar secretions, the result of the inflammatory process. This is opposed to the opinion of the late Professor Rudolphi, who maintained the opinion of Bichat, that the vessels usually supposed to be in the serous membrane are really in the parts they cover, and that the supposed products of serous inflammation are not derived from the membranes themselves, but from the subjacent parts, and therefore are merely exuded through these membranes.

From the anatomical details it is evident, says our author, that the vessels of the serous pleura in the one case, and of the serous pericardium in the other, becoming loaded with red blood, secreted *first* the albuminous fluid whose concretion formed the cellular membrane interposed between the inflamed pleura and pericardium, and the adventitious stratum of muscles, and *afterwards* a fibrinous fluid, the basis of the muscular stratum itself.

As it is impossible to conceive that the functions of muscles could be carried on without the agency of nerves, our author very properly takes their presence for granted, and supposes that they accompanied the vascular ramifications before spoken of. To me it appears probable that both nerves and vessels were generated in these adventitious parts in the first instance, independently of the general vascular or nervous system, with whose extremities, however, they afterwards formed a junction. Our author next proceeds to investigate the uses of these adventitious muscles in the two cases he has described, at the same time observing that they are entirely without precedent in the annals of pathology. In the case of the diseased heart, we have seen that organ affected by a double lesion, softening of some of its muscles, and a conversion of others into fat. It was not the greasy degeneration described by Laennec

as an infiltration of the muscular substance with matter presenting all the chemical and physical properties of grease, but was a true conversion of a portion of the parietes of this organ into fat. To conclude, I may state in a few words, that Dr. Leo. Wolf has proved in a most conclusive and satisfactory manner that these adventitious muscles were destined to repair the ravages committed by disease, being in the first case intended to supply the muscular action lost by the softening and fatty degeneration of the heart, and in the second case being, as evidently calculated, by operating on the ribs and diaphragm, to assist powerfully in increasing the respiratory motions of the empyematous side of the chest, as long as the lung on that side continued capable of performing any portion of the respiratory function, which was probably for several months, so slow was the progress of the disease towards the final and complete carnification of the lung.

The next monograph to which I shall briefly advert, is extremely interesting, but is written in so condensed, or rather such a tabular form, as to render it incapable of abridgment. It is an Academical Program, or Thesis, published at Erlangen in 1832, by Doctor Michael Jaeger, on the subject of operations in which portions of bone were excised. It is entitled *Operatio Resectionis Conspectu Chronologico adumbrata*, and it contains a chronological list of all such operations, together with the name of the operator, the nature of the accident or disease which rendered the operation necessary, and the result of the operation. The labour of compiling this short treatise must have been immense, and could only be achieved by German zeal and industry. A synopsis of the results of some of these operations cannot fail to be interesting to practical surgeons.

In the first page the reader is rather startled by the title given to the first group of operations, *Decapitatio*, printed in large letters; but it is only decapitation of single bones, what we term in English excision of joints, which synonym is given by the author. He first enumerates all the cases in which the articulating extremity of the lower jaw bone has been excised, and it appears that at the time his monograph was published, this operation had been performed one-and-twenty times, viz., eight times in Germany, twice in Paris, twice in America, twice in Copenhagen, three times in Scotland, and four times in Dublin.

Of course this list is not complete, and I am aware that other cases might be added to it both in Dublin and elsewhere. Still, as far as it goes, it furnishes some interesting facts. Of these twenty-one cases, fourteen, that is exactly two-thirds, had a successful termination. This result is the more encouraging,

as in several of those very cases nearly one-half, in others one-half, and in the rest again nearly the whole of the lower jaw bone was removed. Of the fourteen successful cases, nine were cases of *osteo-sarcoma*, two are marked *caries profunda*, two were injuries from gun-shot wounds, and one was a case of necrosis. Almost all the unsuccessful results were where the excised bone was affected with fungus hæmatodes, or some analogous disease.

This formidable but useful operation was first performed by an Austrian surgeon named Fischer, who excised nearly one half the lower jaw bone in a wounded soldier in 1793. In 1799 the same cause led Mursinna to perform the operation at Berlin, and he successfully removed nearly the whole of the bone.

In 1820, Palm, at Ulm, performed the operation unsuccessfully in a case of osteo-sarcoma, having previously tied the carotid artery. In 1821, Gräfe, in Berlin, performed this operation in a case termed a cancerous degeneration of the bone. With respect to this operation of Gräfe, our author calls the result *eventus dubius*; but if I recollect rightly, Gräfe's case turned out unsuccessfully, and my recollection of the result is corroborated by the reviewer in the *Edinburgh Medical and Surgical Journal*, vol. xxvii. p. 318. Gräfe also tied the carotid previous to the operation, as was done by Mott, an American, who performed the next operation of this sort in 1822 in a case of osteo-steatoma, also unsuccessfully. The next in the list is Surgeon Cusack, of Dublin, and to him is due the honour of being the first who exarticulated and excised a large portion of the lower jaw bone successfully in cases of diseased bone, for we have already seen that it had been twice before performed successfully in cases of gun-shot wounds; Mr. Cusack, too, was the first surgeon who ventured to perform this operation without previously tying the carotid artery, as had been done by Palm, Gräfe, and Mott. This was a most important improvement, and probably contributed materially to Mr. Cusack's superior success, and also to the success of others, for it is remarkable, that the carotid was not tied in any of the successful cases. These facts, I think, are sufficient to entitle us to claim for Mr. Cusack the merit which our author gives to Gräfe in the following note:—"Ab annorum ratione si recesseris, et re ipsâ spectatâ, Graefe hujus operationis auctor et inventor habendus est."

Excision of the sternal extremity of the clavicle has been performed three times with success; and excision of the head of the humerus has been very frequently performed both on account of gun-shot wounds and disease of the bone. In cases of

wounds and fractures the result has been very encouraging, even under circumstances apparently desperate, as where it was necessary at the same time to cut off portions of the clavicle and scapula. When this operation was undertaken for the cure of disease of the humerus, which is stated in all the cases to have been caries, it proved successful in eleven cases out of twelve.

The list of partial or total excision of the elbow joint on account of caries of the heads of the bones amounts to thirty-four, of which only six or seven were unsuccessful. The first operation of this nature was performed by Justamond in the last century, and the second by Moreau, who succeeded, after amputating the articulating heads of the humerus, radius, and ulna. The first operation performed in Great Britain was by Mr. Crampton at the Royal Infirmary. The ten first operations on the elbow joint were all performed by French surgeons; Mr. Crampton's was the eleventh.

Exarticulation and excision of the head of the femur has never been performed. Our author quotes the authority of Dietz to prove, that it had succeeded at the Meath Hospital in the hands of the late lamented Mr. Hewson; but all those who were present at the operation referred to are aware that insuperable difficulties frustrated the attempt, and the patient died a short time afterwards of phthisis. In a case of morbus coxæ, attended with a spontaneous luxation of the femur, the head of this bone was successfully removed by Schmaltz at Pirna.

If the Germans lay claim to being the first performers of exarticulation of the lower jaw bone, and the French of excision of the elbow joint, to English surgery is due the honour of the still more daring operation, excision of the knee joint in cases of white swelling. It was first performed by Park in 1781 with perfect success, for the patient regained the complete use of the limb, *membrum præstitit usum completum*.

Park's second operation was likewise successful, although no perfect ankylosis ever took place between the extremities of the divided bones. This operation was five times attempted by other surgeons, but without success, except in the case of the operation performed by Moreau, jun. in 1811, the result of which is marked *usus membri incompletus*. In 1823, Mr. Crampton performed it at the Meath Hospital. Ankylosis did not take place, and the patient died in three years after it had been performed. In the same year Mr. Crampton again performed this operation on a young woman affected with white swelling of the knee joint, and perfectly succeeded, as the resulting ankylosis was complete; and I have seen the patient many years afterwards walking stoutly, so that in this instance

Mr. Crampton has the honour of being the second surgeon who performed this operation successfully. On the whole, it has been performed thirteen times, of which seven, or rather more than one half, were attended with success.

Under the title *Decapitatio Ossium in Articulo Pedis*, our author marks two sub-divisions, *propter cariem*, and *propter luxationem pedis complicatam*; of five belonging to the first subdivision four succeeded; and here again we find Moreau the first person who performed the operation. Twenty-three cases are given belonging to the second subdivision, in which it appears that only one was unsuccessful. The first operation of this kind was performed by the celebrated Hey, in 1766. Under the head of *Decapitatio Ossium Metacarpi* five successful operations are quoted, two of which were performed for the removal of carious bone, and three for the removal of dislocated bones. With respect to the metatarsus, the operation of decapitation was performed five times successfully for the removal of carious bones, and here again the name of Moreau occurs as the first operator, and once for the removal of dislocated bones.

Our author next gives a list of all those operations in which parts of bones not connected with joints were removed. The first section, *Excisio Maxillæ Superioris et Processus Alveolaris*, contains a list of twenty-six operations, of which five were unsuccessful. The two first were performed by French surgeons, Beaupreau and David, in the year 1769.

We next come to *Excisio Maxillæ Inferioris Partialis*, of which he gives a list amounting to sixty-eight. Of these forty-eight, or about five-sevenths, were successful. The first performance of this operation is justly claimed by Denderick, an American surgeon, who performed it with success in 1810, for the account which is given by Anel and Fauchard of its having been performed in 1700 by a Parisian dentist is too vague to deserve attention. The account of Denderick's operation was not published until 1823, therefore the merit of originality is also due to Dupuytren, who performed it in 1812 with success. We next find the name of Sir Astley Cooper, who in 1817 and 1818 removed exostoses from the lower jaw bone; of his operations our author says, *excisio exostoseos proxime ad ipsam resectionem accedit*, so that we must give to Mr. Crampton the honor of being the first British surgeon who removed a portion of the lower jaw bone, which he did in 1818, in a case of osteosarcoma. Dupuytren has performed this operation seventeen times, and was unsuccessful in only two cases! More than half the scapula has been removed by operation in five different instances, of which four were successful. The first attempt of

this kind was made in 1820 by that scientific and bold surgeon Mr. Liston, whose name, together with that of Mr. Syme, occurs so frequently in our author's list. Both these gentlemen have contributed materially to the improvement of British operative surgery, and their names are recorded by all foreign authors with respect, although at home some of their less enterprising and successful cotemporaries have endeavoured to depreciate their merits. In two cases have extensive portions of the sternum been removed by operation, and in both successfully. This was first attempted by Cittadini, an Italian surgeon, in 1812, and again by that eminent operator, Dietz of Nuremberg, in 1830. Under the head of *Excisio Costæ Partialis*, we find enumerated fourteen operations, in some of which portions of two ribs were removed. The first performer of this operation was the celebrated Galen, who thus succeeded in removing a portion of a carious rib, and saved his patient's life. Riverius asserts that a similar operation was performed by Aymar, and with the same result. In 1796 Percy removed with success the sternal extremities of the fifth and sixth ribs, which had become carious in consequence of a gun-shot wound; and in 1812 Cittadini excised the cartilaginous portions of the sixth and seventh ribs, which had become spontaneously carious. To Cittadini is certainly due the honour of having first demonstrated in a clear and intelligible manner the nature of the cases which required this operation; and he first pointed out under what circumstances it might be performed with a probability of success. He himself performed it five times successfully; of the remaining nine operations five were likewise successful. I look upon the operation of excision of portions of carious ribs and of the sternum to be of very great practical importance; and I am sure I have seen cases where it might have been performed with advantage in this city. From the remaining lists of operations on bones which our author gives in full detail, it is not necessary to make extracts, as they do not lead to conclusions likely to be overlooked by any practical surgeon of moderate experience or information.

To conclude, I may observe that our author himself is a distinguished surgeon and most successful operator.

The following short abstract of a very curious case of enormous enlargement of the breasts, is taken from Meckel's Archiv, April, 1830. The appended references to cases of a similar nature will, no doubt, prove interesting to the English reader, being in general taken from sources not accessible to him; we owe them to the industry of Professor Cerutti, of Leipzig:—

A young woman, of a pale countenance, slender form, and

phlegmatic temperament, had enjoyed an uninterrupted state of good health until her 25th year, when she became pregnant. It is remarked that her breasts were naturally large and soft. No unusual occurrence followed delivery, except that the child could not be brought to take the breast, and consequently the mammae became distended with milk, and far exceeded their natural size. It is not stated whether they had regained their usual dimensions before she again become pregnant, about two years afterwards. Be this as it may, both had attained to such a magnitude before the sixth month of pregnancy had elapsed, that she sought medical aid, and informed Doctor Cerutti that about four months previously she had received a blow on the *right* mamma, shortly after which the *left* breast became evidently larger; but its enlargement was unattended by the least heat, pain, or any other symptom of local inflammation. In the course of a few weeks the right breast began likewise to increase in size, but not so rapidly as the left. When first examined, they were so greatly enlarged and heavy, that their weight alone proved a serious incumbrance to the patient; they were both equally hard, and the strongest pressure on them did not produce the least pain. The skin covering both was perfectly natural, and she complained of no uneasiness except occasional stitches darting through the left mamma. The enlargement of the breasts continued to increase until the end of the eighth month, when she was delivered of a dead child on the 15th of March, after which their size remained stationary, and the stitches in the left breast ceased altogether. After some time she commenced an alterative course of mercurials and antimonials, which seemed to improve her general health and made some impression on the breasts, for the right was evidently diminished in size. Both, however, were still hard, but in some spots the hardness was so far diminished as to yield somewhat to the finger when pressed. Under the same treatment these soft spots seemed to increase in size and number, and at last imparted an evident sense of fluctuation to the finger; at the same time her lower extremities, and afterwards the integuments of the abdomen, became oedematous, and in the course of a few days even her face and hands were somewhat swollen, particularly in the morning. The appearance of the oedema was accompanied by febrile symptoms, which, together with the anasarca, speedily yielded to antiphlogistic treatment. The left mamma had now become quite soft in every part, and in fact felt like a bladder full of water; its weight and the disagreeable fluctuation of the contained fluid rendered it extremely inconvenient to the patient, and accordingly it was resolved to let out the fluid, which was effected by means of scarifications

made on the 14th of April, and repeated eight days in succession. This afforded exit to several pints of water, and caused so great a decrease in the size of the left mamma, that it was no longer much larger than the right. The flow of water through the wounds continued for several weeks, until, indeed, the appearance of the left breast was so entirely altered, that it now resembled a flaccid nearly empty bag, containing the mammary gland somewhat increased in size, and of a stone-like hardness. The alteration in the left breast was less perceptible, nor was it evident that it ever had contained any water; like the other, however, it too had become more flaccid. In neither did she feel the slightest pain even on pressure. The use of iodine ointment, and other remedies, had before the end of July effected a still further reduction in the size of her breasts, which, although still much larger than those of other women, and still exhibiting a remarkable hardness of the mammary glands, yet formed no serious impediment in the performance of her usual occupations. So matters continued for thirteen months, when she became a third time pregnant, and in the course of a few weeks the breasts again began to increase in size, and that with such rapidity, that in the beginning of the following April, the left breast presented the following measurements: circumference at basis, forty inches; from nipple to upper border of tumour, twenty-seven inches; to lower, sixteen inches; the right breast measured an inch less in each direction. These enormous tumours hung pendulous over the abdomen, and entirely prevented her pregnant condition being remarked by the eye, although she was within six weeks of her confinement. In some parts the skin, hitherto natural, seemed distended, ready to burst, and painful. The success of the scarifications on a former occasion induced her medical attendants to try them again, but it was now found that very little fluid came from the wound, which immediately became gaping, and exhibited a protrusion of the parenchymatous substance of the breast, firm and fat-like, which protruding portion rapidly increased in size, until it resembled a steatomatous tumour as large as a goose egg. The size of the breasts continued to augment daily, and before the period of accouchement, which happened on the 10th May, 1828, they certainly must have together weighed twenty-four pounds. Their heat was above the natural standard, and here and there their surface was traversed by turgid and swollen veins. They were every where elastic, and in no part uneven or rugged from the occurrence of knotty tumours or hard spots. The integuments were more distended towards their inferior and most pendent portion, on account of the gravitation of the fluid to that part.

In consequence of this the inferior parts yielded much more to the finger when pressed, than the superior, and imparted more of the feeling of softness, but nevertheless they did not pit even on strong pressure. The breasts were narrower at their basis than in other parts, and consequently had a pyriform shape; by pressing strongly against each other they had occasioned mutual excoriation and ulceration on their internal surfaces. At the end of her pregnancy another tumour appeared in the right axilla, about the size of the fist. This was at first painful, soon softened, suppurated, and broke; notwithstanding these various sources of irritation, her general health appeared unaffected, and there were no pectoral symptoms or pains whatsoever. A few days after delivery, the breasts began to diminish in size, and in the course of a week the diminution had so far advanced, that the skin covering the tumour, instead of being distended, was wrinkled and loose; for some weeks before and after the birth of her child, the patient was prevented from sitting up in bed, by the pain in her breasts which the change from the horizontal posture occasioned; when it was absolutely necessary for her to sit up or stand, she could only effect it by aid of persons employed to support her breasts with their hands; and when she remained for any length of time sitting, she was obliged to draw her knees upwards, so as to give support to the breasts which hung over and covered the whole abdomen. In a few days after her accouchement she obtained much relief from the bursting of the abscess in her armpit, which discharged a very large quantity of a white, ropy, milk-like, fluid. On the 30th of June, she was able to follow her usual occupations, and although the breasts were still uniformly hard, and so large as to hang far downwards over the abdomen, yet they were amazingly diminished in size, and the integuments covering them hung loosely and in folds. She could then lie comfortably on either side, and suffered no pain; although still emaciated, she was in other respects healthy. On the 7th of September, she applied for assistance on account of the ulceration between the breasts which had never healed, and on account of the non-appearance of the menses since her confinement in May; she appeared pale and cachectic. Our author determined to try the effects of animal charcoal,* which was administered in doses of half a grain, gradually increased to a grain and a half three times a day; in the course of a month the size of the breast had con-

* Animal charcoal has been strongly recommended by Dr. F. A. Weisse, in indurated tumours, scirrhus, &c. &c.

siderably diminished, and the ulcerated parts had assumed a much healthier appearance, and were healing. Various circumstances, however, prevented her from attending the dispensary, and consequently all remedies were laid aside. She was again examined on the 17th May, 1830, when the left breast, which was still somewhat the larger of the two, was found to measure 21 inches in circumference at the basis, and 9 inches from the basis to the nipple. The substance or parenchyma of the breasts is soft, and the integuments are quite flaccid, loose, and pendant, so as to afford proofs of the former enormous size of the parts they covered.

Our author proceeds to remark that in none of the systematic works published in Germany upon diseases of women, is any description given of a disease of the mammæ similar to that just described; nevertheless he observes that several cases of a similar nature may be found scattered through the works of physicians both ancient and modern; *Galen* in speaking of an unnatural increase in the size of various parts, such as the scrotum, the tongue, &c., says that he has seen a similar affection of the mammæ, in which their size is enormously increased without any previous pain, inflammation, induration, or dropsy. In such cases he says, that although the breast retains its form, it loses its function of producing milk, and he affirms that this disease is oftener congenital than acquired. After the time of *Galen*, this hypertrophy of the breasts although known, was not exemplified and illustrated by new cases and descriptions, until more modern times, when the improvement of medical science received a new impulse. *Scaliger** describes briefly a case of this nature, and *Bartholinus*† speaks of a woman whose breasts were so enormously enlarged, that they hung down as far as her knees, upon which she used to support them when she sat down.

Palmuthius‡ relates a similar case: a woman had breasts rather larger than usual even before marriage, but their size began notably to increase during her first pregnancy, and also during each succeeding pregnancy, until they at last acquired enormous dimensions hanging, down as far as her knees. This case of *Palmuthius* resembles therefore that above described by *Cerutti*, in the period of increase of size being that of pregnancy, and it likewise presents another point of similarity, viz. the appearance of tumours in the axillæ.

* *Eercitat. ad Subtilitatem Cardani*, 149. 2.

† *Jo. Schenk, Obs. Med. Lib. 2. p. 304. ed. 1665.*

‡ *Histor. Anatom. Cent. iii. obs. 46, quoted in Meckel's Archiv für Anat. v. Phys. 1830.*

In Welser Augsburgh Chronicles, (1591, tom. iii. p. 104,) we have the particulars of a very curious case. A servant maid was so incumbered by hypertrophy of the mammae, that she could scarcely either stand or walk, in every other respect her health was good, the left breast was successfully amputated by a barber, and was found to weigh twelve pounds; the young woman was so relieved by its removal, that she was able to support the burthen of the right breast without any great inconvenience. A lady near Koningsberg* had a similar affection; her breasts were so enormous, that one of them alone weighed nearly thirty pounds, and the patient was obliged to have recourse to suspensory bandages tied round the neck to enable her to support them. In this lady the removal of a suppression of the menses under which she had long laboured, was effected by judicious treatment, and an immediate diminution of the size of the mammae was the consequence of the restoration of a healthy state of the menstrual discharge. A lady of rank† who had previously enjoyed a most uninterrupted state of good health, produced a suppression of the menses by incautious exposure to cold during the menstrual period, immediately her breasts became painful, and began to swell, and had so increased in size during the following night, that she could neither get out of bed or move herself. She was twice bled in the foot, and, the menses were restored thereby, and the affection of the breasts entirely removed.

Dornsten‡ observed a very remarkable case of hypertrophy of the mammae, and his description of the disease is far better than that of his predecessors; a girl, twenty years of age, perfectly healthy, and not very slender, awoke one morning out of a sound sleep in a fright, and was astonished at finding both her breasts so enormously enlarged, that their size and weight prevented her from changing her position in bed; she did not feel either pain or soreness in the swollen parts, nor was she otherwise unwell. The lactiferous ducts were hard and distended, but there was no appearance of the swelling being occasioned by either emphysema or anasarca. The left mamma measured 31 inches, the right 38 inches in circumference. This attack commenced in July, and terminated fatally in October, the young woman being worn out by other symptoms, for a detail of which I beg leave to refer the reader to the Philosophical Transactions. After her death, the left breast,

* Borrelli, Hist. et Observ. Cent. i. obs. 48.

† Ephem. Nat. Cur. Cent. i. et ii. obs. 67.

‡ Philosophical Transactions, 1669. p. 1047.

which had continued to grow since July, was cut off, and weighed 64 pounds. It was accurately examined, and found to contain within its structure no serous or aqueous fluid, and indeed nothing morbid or unnatural; its uncommon size seemed owing altogether to simple hypertrophy of the parenchyma of the breast. The left breast was not removed, but it must have weighed about 40 pounds; this case made a great noise at the time it occurred and was seen by many medical men; it may be observed that the entire weight of the two breasts amounted to about seven stone and a half, which is not far short of the weight of many moderately sized women.

Sauvages, in his *Nosologia Methodica*, tom. iv. p. 305, mentions a case of a nearly similar nature, which occurred at Toulouse; and Hey, in the 14th chapter of *Practical Observations on Surgery*, relates the following: a girl 14 years old, slender but healthy, was remarkable, even from an early age, for having breasts a good deal larger than natural. The catamenia, which appeared when she was thirteen years old, were suppressed by exposure to cold, and were never afterwards restored; her breasts immediately after the suppression began to grow, and increased in size from day to day with such rapidity, that when seen by Hey their weight was insupportable; amputation of the left breast was performed, and in a short time afterwards, the catamenia re-appeared, and became regular. The remaining breast now began to diminish in size, and in six months was not more than half as large as formerly. For a more accurate description of this case, so remarkably illustrative of the sympathetic connexion which exists between the uterus and mammæ, I must refer to Hey's work.

Osiander* saw a girl in whom one breast only was increased in size, and had attained to enormous dimensions, but as it does not appear to have been a case of simple hypertrophy, it deserves no further notice, in treating of the present subject. Jorden† relates a case very like that described by professor Cerrutti: a woman of a phlegmatic temperament, and 26 years old, had borne three children, and during each pregnancy her breasts increased in size, and at last became quite enormous. After each accouchement, a milk fever set in on the 4th day, and during its continuance, the breasts were still further enlarged. In about three weeks, however, they had again decreased, and soon became much smaller than they had been during the pre-

* *Denkwürdigkeiten für die Heilkunde und Geburtshülfe.*

† *Hufeland's Journal*, tom. xiii. with a copperplate.

vious pregnancy. The regularity of these changes was very remarkable.

My friend professor Dzondi* of Halle, is also cited as having published an account of an hypertrophy of the mammae, in which it was necessary to remove one of the breasts; it weighed 22 pounds, of which nearly seven pounds consisted of a serous fluid. The structure of this breast was much more compound than it is observed to be in cases of simple hypertrophy, and it contained a vast number of cavities or sacs, of various dimensions.

The next monograph I shall notice, relates to the anatomy of the Eye, and is entitled,

On the Zonula Ciliaris in the Human Eye, by Dr. M. I. Weber, Professor Extraordinary of Comparative and Pathological Anatomy, and Demonstrator at Bonn, Member of several learned Societies.†

Amongst the most distinguished anatomists there still prevail different opinions as to the true nature of the ciliary zonula (corona, lamina ciliaris, zonula Zinnii).

Any thing which contributes to the clearing up of this subject must be an acceptable offering to the anatomy and physiology of the organ of vision, even should it belong to the province of irregular formations of these organs, as in the present instance. This contribution appears to me to possess, indeed, more value on this very account, for it makes us acquainted with a form of disease of the zonula ciliaris which incontrovertibly proves this part to be a peculiar and independent organ.

The zonula ciliaris has been considered by some as a continuation of the retina, by many, and these are the majority, (amongst whom at the present day are Meckel, Hempel, Böck, &c.), as a continuation or lamella of the membrane of the vitreous humours; Rudolphi first and Doellinger (seventeen years afterwards) are the only two who have declared it to be an independent membrane.

It is manifest from the descriptions of Lieutaud, Cassebohm, and particularly Ferrein, that they held the zonula to be a continuation of the retina. But Zinn‡ and afterwards Ru-

* Beiträge zur Vervollkommenung der Heilkunde. Ir. Theil. s. 91.

† This monograph is dedicated by the author to his friend Dr. F. Nees von Esenbeck, as a birth-day present, 26th July, 1827, a custom very general in Germany, and one which bespeaks that mixture of primitive simplicity of manners and intellectual refinement which still forms a part of the national character.

‡ I. G. Zinn Descriptio Anatomica Oculi Humani. Goetting, 1755, p. 114.

dolphi,§ have cited several cogent reasons against the adoption of this idea.

Zinn says, "Plurimi tamen, si eorum verba paullo magis attendamus coronam illam ciliarem inferius describendam, sive zonulam tenerrimam fibrillis brevioribus strictam, quæ ab humore orta convexitate anteriori lentis inseritur, pro ipsâ continuatione retinæ habuisse videntur, quod inprimis ex descriptione Lieutaud, Cassebohmii et Ferreinii apparet, qui retinam ad aliquam a lente distantiam teneriorem et pellucidam factam in convexitatem anteriorem capsulæ lentis immitti et ad insertionem usque deduci posse affirmant. Nec membranam communem Paluceii, quam retinæ partem, et ab involucro nervi optici natam esse scribit, ut fibras medullares instratas sibi habeat, aliud quidquam esse quam zonulam coronæ ciliaris, data illius descriptio optime docet quum et sulcos, qui plicas processuum ciliarium excipiunt, et ejus in convexitatem anteriorem insertionem in illâ itidem animadverterit, etsi, non satis accurate locutus, illam cum ipsâ retinâ confundere, et pro illius propagine habere videatur. Sed et alia sunt, quæ dubitationem aliquam relinquunt, et assensum meum fere morentur ut circa insertionem retinæ in lentem nondum plané sim persuasus. Dum enim in oculo recenti ubi et choroides et retinæ solidiusculæ sunt, choroidem et illi continuum corpus ciliare detraho, ipsa retinâ ad originem processuum ciliarium margine tumidulo inprimis in oculo bubulo, terminatur, cujus oram in ove arteriola in circum ductum legit ut in bove integerrimo margine elevari, et revolvî possit, et eloto muco nigro, inter illum marginem tumidulum et lentem humor vitreus appareat. Præterea si ad lentem pertingeret, aut inter choroidem et annulum nigrum, aut inter annulum nigrum et humorem vitreum illuc deferri necesse foret. Inter corpus ciliare et annulum nigrum non produci videtur, quam viam omnino intercipit mucus niger in zonulam continuam conflatus, quem ex ultimis villis choroidis, minime autem a retinâ secerni, quilibet facile concesserit. Sed neque inter annulum nigrum et humorem vitreum deferri crediderim. Annulus enim niger transpiciendo per humorem vitreum aterrimus semper apparet, nec nullâ omnino nebulâ tutus, aut levissime dilutus conspicitur, sed marginem anteriorem retinæ, per quam semipellucidam choroides obscurius pellucet ad originem processuum ciliarium, non sensim evanescere, sed fine ubique æquali et accuratè limitato terminari videmus. Præterea omnis fere analogia corporis humani repugnat, quum nullum sit in corpore humano exemplum, extremitatem cujusdam nervi pulposam, medullarem mutari in membranulam pellucidam, quæ omnis sensus expers, aut partem involvat, aut ligamenti in-

§ A. Rudolphi, Anatomisch-physiologische Abhandlungen. Berlin, 1802.

star inserviat. Experimenta autem in oculis affuso alcohole servatis instituta, paulo minus valent, quod ipso alcohole omnia ita opaca redduntur, et constringuntur, ut retinam neque ab humore vitreo et zonula coronæ ciliaris satis distinguere, neque sine læsione auferre liceat. Quæ quum ita sint, non possum quin iis adstipuler, qui retinam ad originem corporis ciliaris terminari perhibent."

A. Rudolphi has brought forward in his "Anatomisch-physiologischen Abhandlungen" three reasons to shew that the zonula cannot be a continuation of the retina. These are—

- a. When the retina is taken away, the canal of Petit remains still closed. But, Rudolphi justly remarks, this proof which Zinn and Hildebrand cite as demonstrative is not sufficient. It might be alleged by opponents that the zonula posteriorly upon the vitreous humour, where it arises from the retina, is firmly connected with the hyaloid membrane, somewhat as the choroid is connected with the sclerotica by means of the ciliary ligament.
- b. The zonula is much thinner; and when it is said that it is a lamella of the retina, this should be demonstrated, as also what becomes of the other lamella.
- c. Its whole structure is different. It is not medullary, and is therefore not like the retina coloured yellow by dilute nitric acid. If it be said that the retina here lays aside its medulla, this remains unproved.

There is a fourth reason of no less weight which I would here call attention to, namely this, that the *retina is not only not intimately united or incorporated with the zonula ciliaris*, inasmuch as it can be separated from it with the greatest facility, and without the least force being employed, but also that the *retina at the outer circumference of the ciliary zonula turns, or rather is reflected internally, that is, towards the vitreous humour*. I found this turning up of the retina, which may amount to about half a line, in the eyes of a child ten or twelve weeks old, and partially also in animals, particularly in calves' eyes. The connexion of the retina with the zonula is of different strength in various animals; but in none does there take place that intimate union, or rather melting together, of the two parts.

The turning back of the retina can probably only be demonstrated in the eyes of the foetus and very young subjects, as I have never remarked it in those more advanced.

Those who hold the zonula to be analogous to the hyaloid membrane, and regard the latter as a mere continuation of it, have different opinions as to the manner of its formation or continuation.

Winslow asserts that the hyaloid membrane consists of two

lamellæ, which are intimately united behind, but before separate from one another, and in this manner enclose the lens. Others, as Cassebohm, Petit, Meckel, &c., believe that the hyaloid membrane is separated anteriorly into two laminæ; the external is the zonula, and the internal applies itself to the posterior surface of the capsule of the lens, and between both is the canal of Petit.

Zinn, finally, does not believe in such a partition of the hyaloid membrane, but makes the zonula arise from the hyaloid as a new membrane. He says, "*Nunquam autem ex capsula lentis flatus in eum penetrat, uti necessario contingeret, si annulus ille et capsula lentis duplicaturâ tunicæ vitreæ formarentur, etsi vel ipsa insertio hujus zonulæ huic opinioni repugnet, quæ non in circulo maximo, sed in convexitate anteriori sese ad lentem applicat. Sed neque a retinâ pellucidâ factâ parietem hujus annuli anteriorem formatum fuisse vel inde apparet quum optime tunc demum annulus ille inflari possit, si retina plane remota et humor ater corpus ciliare oblinens abstersus fuerit. Annon autem ad veri speciem proprius accidet ex tenerrimâ tunicâ vitreâ novam hic ortam fuisse membranulam, quam si quis persuadere nobis vellet, tunicæ vitreæ lamellas duas, posterius sibi agglutinatas antè a se invicem ad constituendum illum annulum secedere, cum nullibi vel levissimum vestigium duarum lamellarum observari possit, neque ab ullâ arte humanâ in tunicâ vitreâ duas demonstrari posse lamellas, exspectari posse videatur? Flatus demum corpori vitreo immissus illud undique sic elevat, ut illius faciem quoque anteriorem, ubi sinu lentem excipit, tunicâ propriâ obduci facile appareat.*"

But Rudolphi brings forward also against this idea five valid arguments, which Doellinger has strengthened:—

- a. The zonula is thicker than the hyaloid itself, of which it is stated to be only a lamella.
- b. If the eye be put into spirits of wine, the zonula immediately loses its transparency; but this does not happen with the hyaloid.
- c. The zonula is obviously coherent with the retina.
- d. It can be separated from the hyaloid without injuring the latter, where it is said to arise from it.
- e. In some animals, especially birds, the posterior edge of the zonula, where it is connected with the retina, projects over the hyaloid.

Doellinger* says, "*Plerique a Winslowi inde tempore hanc coro-*

* J. Doellinger, *Illustratio Ichnograph Fabricæ Oculi Humani*. Wiruburgi, 1817, p. 15.

nam ex diffissione hyaloideæ prodire putant, ast plura sunt huic divulgatæ opinioni contraria :—

- a. Hyaloidea omnino tantâ gaudet tenuitate, ut quâ ratione in duas laminas dividi possit, omnem mentis aciem excedat, recte sic etiam monente Zinnio.
- b. Coronæ ciliari longe alia ac quidem hyaloideæ inest structura, ex apertis enim fibrarum fasciculis tenuibus lamellis conjunctis et radiatim dispositis constat, hyaloidea autem nihil fibrosi habet.
- c. Hyaloidea ad posteriorem lentis convexitatem non est tenuior quam in aliis oculi regionibus, quod tamen contingeret, si re ipsâ diffissio facta esset.
- d. Coronam inter et hyaloideam jacet retinæ continuatio ciliaris, ita ut corona ciliaris ne communicationem quidem cum hyaloidea habere possit, quare etiam Cl. Zinn errat, dum asserit “ex tunica vitreâ oriri membranulam aut zonulam.”

This last point would be a most decisive argument for the independence of the zonula, if it were really grounded in nature.

Here I call to mind the *reflection of the retina* observed by me, which indeed was no illusion, as this arrangement of the retina appeared without previous expectation on my part. And I can safely assert, that I have examined hundreds of human and animal eyes with reference to this point, both fresh and several days old, and have treated some with spirits of wine, others with solution of sublimate and with dilute acids, but have never any where found the least trace of the slightest continuation under, or rather behind, the zonula.

The most positive proof that this continuation does not exist, is furnished us by treating the retina with dilute nitric acid. The retina may be exposed to the action of nitric acid in any manner we choose, that is, the eye-ball may be divided lengthways or obliquely, or we may take away the sclerotic alone, or the sclerotic and choroid, or the sclerotic and cornea, &c., or we may let the eye lie for two, three, six, nine hours, one, two, three, six, or eight days in dilute nitric acid, and turn the preparation in any manner, and even examine it with the aid of glasses, *the retina will invariably be thickened, and coloured yellow only as far as the commencement of the zonula*; but behind the zonula is no trace of the colouring of the retina, and consequently not the slightest continuation to be discerned. It is well known that the delicate expansion of the auditory nerve is beautifully represented by the action of dilute nitric acid, how much more should this take place here where a similar nervous expansion should be in question.

Doellinger says (in his *Abhandlung über das Strahlenblättchen*, p. 274, of the *Verhandlungen*, K. L. C. Akademie der

Naturforscher, 9 Bd.), "I have found that the nature of the zonula is plainest and easiest discerned, if we cut out a segment from the eye-ball in the neighbourhood of the optic nerve in such a manner that some of the vitreous humour lies free; then lay the eye-ball in a dilute solution of sublimate, and after some time, when the vitreous humour has become opaque, before losing its volume, as would happen in spirits of wine, empty it together with the retina (which easily disengages itself from the tunica Ruyschiana) upon a hemispherical vessel with a dark ground, of the size of the eye-ball. This is very easily effected if the sclerotic be cautiously cut through in the middle of the eye-ball, and if circumspection be used in separating the zonula from the ciliary process." I believe that this very treatment of the preparation may be the means of deceiving the operator. How should we be able to decide as to the attenuated continuation of the retina, when the vitreous humour is become opaque?

But an anormal formation of the ciliary zonula, which I lately observed on both eyes of an infant from ten to twelve weeks old, while the formation of the hyaloid membrane, the vitreous humour, the capsule of the lens, and the lens itself, and also the retina, was perfectly normal, must be a decisive proof of the independent nature of the zonula ciliaris. I proceed to describe this more particularly.

I examined both eyes twenty-four hours after the death of the child, without preparing them in any way, that is, without laying them in water, alcohol, or acids, as I usually do when I have no particular object in view in the examination. The subjoined lithograph will render the description intelligible. I took away from the right eye, first, the sclerotic, and then the choroid. The corpus ciliare was detached nearly spontaneously from the zonula, which immediately appeared to me as a beautiful greyish white, untransparent, roundish dentated ring. I now paid particular attention to its connexion with the retina, and found it to be only slight. I perceived that the edge of the retina, which enters into connexion with the zonula, was thicker than the remainder of the retina, and in separating it from the zonula that it had the same roundish indented form as the external edge of the zonula, and even that both exactly corresponded to one another, or were dove-tailed into each other, somewhat like the serrated edges of the bones of the skull.

When the retina was free all round, the *returning of the retina* above remarked immediately shewed itself. I believe that this reflection of the retina at an earlier period was incorporated with the inner surface of the retina, and was thus, as far as I am aware, overlooked.

I divided the left eye by an oblique cut through all the





Drawn on Stone by William Lever after the German

Albus Lithog. 6 Trinity St.

membranes and the vitreous humour into two halves. After I had with facility taken out the vitreous humour with the crystalline lens and the zonula, the retina remained still lying on the edge of the corpus ciliare, and the reflection of the retina was exhibited still plainer than before, as I moved the preparation carefully backwards and forwards in clear water. (*See Plate.*)

Magnified two hundred and twenty times by means of a compound microscope, the structure of the zonula appeared to be the following: it is a nearly homogeneous medullary substance, in which the formation of globules is beginning, for we remark that the single globules begin to be separated from the general substance of the organ where they are yet partially connected with it. In this commencing structure is remarked a radiated arrangement of the half separated elementary particles. But the fibrous formation by which the radiated structure can be more easily made evident is here entirely deficient. At the points *aa* of the first figure, where the single indentations meet, the radiated structure ceases, and the substance becomes homogeneous, as is also the case generally on the edges *bb*. At the edge of the zonula at *cc*, which meets the lens, the substance is likewise more homogeneous, and somewhat thicker; *dd* is a groove in the substance, in which a vessel must have run, *eee*. The dark marks indicate deepenings of the medullary substance, which probably go out upon the under (posterior) side of the organ, and then are in connexion with the membrana hyaloidea, *fff*. The regular dendritic figures are formed by the pigment which remained on the zonula of the left eye at the taking away of the corpus ciliare. The bright edges which are universally remarked on the side of the pigment are remains of the tunica Ruyschiana, which encompasses the pigment. The radiated arrangement is not always conformed in large rays, as is partly the case in *aa*, but also there are formed sometimes intermediate systems, such as are remarked at *b*.

The retina which I examined in comparison with the zonula, as well in these eyes as in the eyes of animals under similar relations, that is, with a two hundred and twenty times magnifying power, is distinguished by a greater thickness and by the deficiency of the radiated arrangement of the zonula from this structure.

The second figure represents the zonula in the natural size: *a*, the lens; *b*, the limits of the vitreous humour with the perfectly transparent and unorganized membrana hyaloidea, which runs immediately under or internal to the retina, as far as *c*, the edge of the zonula, and then proceeds under the retina, and surrounds the cavity in which the lens, with its capsule, lies: *c*, the serrated edge of the zonula; *d*, the part of the zonula which

lies towards the lens. I owe the drawing and the microscopic examination to the kindness of Dr. Meigen.

Finally, then, I announce, with Rudolphi and Doellinger, the zonula to be a peculiar independent structure of the organ of vision, and support the opinion of these celebrated anatomists by two observations:—

1st. By the anormal formation of the zonula.

2nd. By the reflection of the retina.

This memoir is evidently the production of a practical anatomist, who has attentively investigated the subject which he discusses, but he does not appear to have examined the observations of Camper, in his essay, *De quibusdam Oculi Partibus*, of Hovius in his work *De Circulari Humorū Motu in Oculis*, of Monro in his work on the Brain, the Eye and the Ear, or of Dr. Jacob in his paper in the 12th volume of the *Medico-chirurgical Transactions*. The latter celebrated ophthalmic anatomist and surgeon, relying upon accurate dissections of the eye, and fortified by the authority of the writers above alluded to, and especially by the representation of the *zonula* by Hovius and Monro in their plates, considers this part of the hyaloid membrane called *corona ciliaris* or *zonula Zinnii*, which is between the termination of the retina and the circumference of the lens, to be the place where an intimate union, absolutely essential to the perfect mechanism of the eye, is established between the choroid and vitreous humor. It is obvious that, in order to render the common cavity of the chambers of the aqueous humor close and perfect, and capable of retaining and confining the fluid which fills it, especially when the organ is subjected to pressure, the sclerotic just behind the cornea must have a firm connexion with the corresponding edge of the choroid, while an equally firm and intimate union must exist between the inner surface of the choroid and the corresponding surface of the hyaloid membrane. This essential, firm, connexion between the sclerotic and choroid is effectually established by the interposition of the circular ring called ciliary ligament, while an equally essential and firm connexion is established between the choroid and vitreous humour, by the ciliary processes. There can be no doubt whatsoever that the part called *zonula Zinnii* or *corona ciliaris*, is a space of anatomical mechanism precisely of the same nature as the ciliary processes or *corpus ciliare* of the choroid, differing only from the *corpus ciliare* of the choroid in delicacy of structure, being nearly transparent, and destitute of red vessels. The *corona* or *zonula* is in fact the provision for intimately uniting the choroid to the hyaloid membrane, a union absolutely necessary

to secure the integrity of the mechanism of the organ. The following is the description given by Dr. Jacob.

“ If the sclerotic, choroid, iris, and retina, be removed one or two days after death, leaving the vitreous humour with the lens imbedded on its anterior part, we observe a number of striæ on the vitreous humour converging towards the circumference of the lens, corresponding in number, size, and form, to the ciliary processes, giving the same appearance collectively that the circle of ciliary process, or *corpus ciliare* does on the choroid, and narrowed towards the nasal side, as the *corpus ciliare* is. This appearance has been noticed by most authors,* but some describe it as arising merely from the marks left by the ciliary process, while others consider these striæ of the same nature as those productions of the choroid, and call them the ciliary processes of the vitreous humour; it is the *corona ciliaris* of Camper and Zinn. If we remove the black pigment with a camel hair pencil, we leave those productions on the vitreous humour more distinctly marked than when covered by the colouring matter, and presenting all the characters above stated; commencing behind with a well defined margin, and terminating anteriorly by attachment to the capsule of the lens, the furrows between them capable of receiving the ciliary process of the choroid, and the folds calculated to be lodged in the corresponding furrows of these processes.”

Doctor Jacob's description of the zonula is so clear and accurate that it leaves nothing to be desired on the subject, and if known to Doctor Weber would certainly have been quoted by him. The view of enabling British anatomists to compare the results of the latest investigations in Germany with the previous conclusions of Doctor Jacob, concerning this interesting point of ophthalmic anatomy, has induced me to publish the whole of Doctor Weber's monograph, for the translation of which I am indebted to my talented and learned friend, Doctor Houghton.

ROBERT J. GRAVES.

* Zinn de Oculo, cap. iv. sect. 3.—Halleri Elementa Physiol. lib. xvi. sect. 17.—Camper de quibusdam Oculi Partibus in Halleri Disp. Anat. Select. vol. iv.—Hovius de Circulare Humorū Motu in Oculis.

Recherches D'Anatomic Transcendante et Pathologique, par
M. SERRES, &c.
Researches on Transcendental and Pathological Anatomy,
by M. SERRES, Paris, 1833, pp. 315.

(Continued from Vol. II. p. 411.)

At the termination of the last part of our analysis of this work we stated the opinion of Serres, that in the progress of the foetal development of the Caucasian variety of mankind, the head progressively represents that of the different varieties from the negro upwards, and pointed this out as an interesting question for phrenological discussion. The head of the negro then represents the early periods of the intra-uterine development of the Caucasian skull. So many important and interesting deductions could be made from this fact, if established, that we would be glad to find it verified by other observers. This leads us to the arrest of development of organs: speaking of the formation of organs M. Serres says,

“ The first period is characterized by the subdivision (*fractionnement*) of organs, and their increase by extussusception; the second by the union of the organic materials, their increase by intussusception, and the different metamorphoses, either in form or position, which their parts experience, before attaining definitively the position and form which they are to present in the perfect animal.

“ It is during this progressive development, according to the beautiful remark of Harvey, that the organs of one class correspond to those of another; and according to our views, of a species more exalted to one lower down in the animal scale. It is this idea which we seek to express, in saying that organogeny often reproduces comparative anatomy.”

After shewing how the persistance of the embryonary condition of organs may induce a pathological state, the author observes, that although most of this doctrine is comprised in the law of arrest of development of Haller and Meckel, yet that the formula of these physiologists, though applying to developments, will not apply to the theory of formation, unless we determine the exact condition of the organs thus arrested in their progress. The rule is, that under these circumstances, organs which should be composite are simple; in other words, that organs which should consist of a number of parts, are presented to us consisting of but few. Thus the osseous system of the cranium reproduces the simple organization of the sacrum. The intestinal canal, when its inferior and superior openings are closed, reproduces the simple form of the serous membrane. So also

with respect to the closure of the pupillary membrane, the orifice of the bladder, &c. &c. In these cases, whether we consider them as in some cases diseases, and in others monstrosities, we see a complicated organ, whose metamorphoses have been arrested, representing the normal disposition of more simple organs, and it is this that is meant by anatomists when they declare, that the imperfections of the organization of man cause it to approach to that of animals.

M. Serres properly remarks, that modern anatomists are far from implying by the above doctrine, that man is convertible into this or that animal. In fact in our comparisons of organs we must take care that we are dealing with identical organs. No human monster, no matter how early or how general has been his arrest of development, can present (with respect to his lungs, for instance) the gills of fish or the air bag of reptiles, because the lung of man is not analogous with either of these organizations. The organs of man, in this description of monstrosity, can only reproduce some of the states proper to them in their intra-uterine existence.

As illustrative of the early periods of organogeny being characterized by subdivision of organs, and of the resemblance produced by arrest of development to the analogous organs of animals, the multilobular disposition of the prostate and kidneys is quoted among other examples. The prostate, simple in the adult, is yet not only double in the embryo, but even presenting more than two lobes on each side. About the fourth or fifth month of intra-uterine life, a fusion of the two internal lobes takes place, and the organ is then composed of but three parts, and at a still further period, these also unite, and form, like the kidney, a single organ. Here we see the two periods of organogeny; the first of formation, characterized by subdivision of organs and the increase of these parts by extussusception; the second, of development, characterized by diminution of the number of parts in an organ, and their increase by intussusception.

"We know further," says M. Serres, "that in various mammiferous animals the kidneys remain multilobular, as we see them in the human embryo, and if this disposition continue in the latter, the organ will reproduce the appearance which it preserves in the former. The same applies to the prostate, an organ which in the sheep, the ox, and the elephant, is formed of several lobes, and in the solipedes in particular is composed of four lobes. If then the embryo of man is arrested at any of its periods, we can understand how the prostate will represent either that of the solipedes, or the pachydermata. In these cases an unusual sinus exists in the prostate, in which calculi may occur, and the sound become engaged in the operation of catheterism."

The prostatic unity of man recalls the uterine unity of the adult woman. This unity, which is preserved more or less perfectly in apes, shews manifest traces of duality in the females of the carnivora, and the herbivora.

“ Finally in some of the latter, as the *Cavia* of *Gmelin*, and particularly in the hare, there are two uteri, completely disjoined, and presenting separate openings. Now in women we find also bilobate uteri, as in the rodentia; or partly divided, as in the females of the herbivora, or with the fundus presenting a deep fissure, as in many apes. These malformations then are nothing but a representation of the state of the organ in beings where its development is less perfect.

“ But all this is nothing but an arrest of formation (*arrest of symmetry*); for the uterus formed of two isolated portions is bicornuous in the embryo of the second or third month. These halves are subsequently united at the neck, and the organ is then simple anteriorly, and double posteriorly, and at last this double portion unites, and constitutes the single uterus of the adult woman. We see, then, how comparative anatomy is reproduced by organogeny, and how and why pathological anatomy, and also monstrosities, repeat the organic dispositions of comparative anatomy. Anatomy is one.”

But of all the instances, illustrative of the stamp of the animal on the human organization, there is hardly one more striking than the caudal prolongation or tail of the little embryo. If there is one character more striking than another, as distinguishing the form of man from most of the quadrumana and the mammalia, it is the absence of a tail. Yet there is a period when the human embryo actually presents this peculiarity, and it is most interesting to observe, that it is at the period of its manifestation and duration that we see the other resemblances to animal organization best marked. It is at this epoch that the osseous subdivisions of the face and skull represent the permanent character of the same bones, in the mammalia, reptiles, and fish. It is at this epoch that the viscera represent in a transient manner the forms of those organs in animals. And we find further, that, like the other organic resemblances to animals, this also is ephemeral; that it disappears after the third month, the time when man oustepping in his developments the rest of organized beings, advances rapidly to assume the superior type which so proudly and pre-eminently distinguishes him.*

* In the fourth volume of the Dublin Hospital Reports, Professor Jacob has recorded an interesting example of the existence of a prolongation resembling a tail, which was attached by a bony union to the extremity of the vertebral co-

A most important consideration here arises, and one of the utmost consequence in practical medicine, it is that many instances of what is considered the result of disease after birth are nothing but examples of arrest of development, of the persistence of a condition natural at some period of intra-uterine life. The organ supposed to be diseased or monstrous, is only so by comparison, in other words it has remained stationary, while the rest of the organization has advanced. When we come to analyse the portion of the work devoted to pathological anatomy, we shall return to this subject, and merely remark here, that we are still sceptical as to the truth of the opinion of Serres, that pathological anatomy, from our neglect of this principle, has been erroneously considered as a science of exception.

We next come to the consideration of the seat of organic deformities, which proceed from arrest of development. It would appear that all organs are not equally subject to these deformities, and the same remark applies also to the different portions of the same system of organs. In this disorder, however, there is still order, the result of the general law of development from the circumference to the centre. Now the principle seems to be, *that the more eccentric the position of the organs of animals, the more variable and different from one another do we find them*, hence the infinite variety of the zoological cha-

lumn, and was removed in the Maryborough Infirmary. Doctor Jacob, however, doubts whether the patient was really an instance of an *Homo ferus caudatus Hibernus*, and inclines to the opinion, that the prolongation was more of the nature of a supernumerary extremity, and presenting a tarsal and metatarsal bone and phalanges.

It is remarkable, however, as Doctor Jacob observes, that it consisted of but one series of bones out of the five. This, and its connexion with the vertebræ, leave some degree of probability that it might have been an example of persistence of the foetal condition above noticed. In his paper this distinguished anatomist and naturalist states, that the records of medical science are by no means destitute of examples of tail-bearing men. He quotes from the *Miscellanea Curiosa Academiae Naturæ Curiosorum*, (Anno 1690, p. 222,) an account of an *homo candatus*, by Emmanuel König, the subject of which was the son of a Doctor of Laws, and whose tail, which was half a span long, grew directly down from the os coccygis, and was coiled towards the perineum, causing much discomfort. Also another case from the same work, (an. 1688,) in a communication by Michael Lochner, entitled, *De Puero Candato*. He quotes another case by Stephen Blancardus, of Amsterdam, and remarks, as a curious fact, that these instances should have escaped the notice of Lord Monboddò, anxious as he was to prove the existence of *homines caudati*. The examples brought forward by Lord Monboddò are evidently drawn from fabulous sources. Doctor Jacob, in conclusion, states, that he has been assured by a person of veracity, who states the fact from actual observation, that he knows an individual who has a production from the coccyx, which can be felt through the clothes, and causes inconvenience when the person sits, and that it is generally believed, that several members of the family have a similar appendage!

racters, which exactly as we approach the centre, we find to diminish. Thus the muscles are more analogous than the external parts, the bones more analogous than the muscles, and the viscera still more analogous than either. Whatever organic system we examine, we find the resemblances in the centre, and the want of them on the surface.

Embryogeny reproduces these characters. We know that the primitive forms of the heart and cerebro-spinal axis is the same in all classes, yet the periphery is so different, that each preserves its proper type.

“ Thus,” says M. Serres, “ the embryo of the reptile does not represent that of the fish, at the period when its brain is ichthyomorphous. The embryo of the bird is neither a reptile nor a fish, at the periods when its brain reproduces the forms of these classes. Lastly, the embryo of man is always man, although his cerebro-spinal axis assumes in an ephemeral manner the encephalic forms of the inferior classes.”

The author draws two conclusions from this principle, 1st, that the peripheral organs will be more liable to deformities than the central; and 2dly, that in the different organic apparatuses the monstrosity will more frequently occur at the extremities than at the centre of the apparatus. That these conclusions are proved by facts he illustrates by a great number of examples.

The next part of the work is dedicated to the investigation of a still more difficult part of the subject, namely the laws which govern the monstrosities by excess. As a constant reference to the plates which accompany this work is necessary for the understanding of this part of the subject, we shall merely content ourselves with giving an outline of the opinions of our author.

When we examine organs which are monstrous by excess, we find them divisible into two classes; first, the double organs, which are fused, and so become complex, and secondly, organs which should be single, presenting a double development, more or less well marked; the term complex organs applies to both these classes, but in the second class, where single organs present the double formation, there must have been originally two uteri, two bladders, two hearts, and so on: it is then only in the true monsters by excess of development that we meet the single complex organs, such as the uterus, the heart, the liver. It is most interesting to remark, that in these double organs, the organic individuality is exactly represented by the arteries; thus, after as before the reunion of the two livers, the two stomachs, the arteries of each preserve their speciality, so

that the complex liver has two hepatic arteries, the complex stomach two gastric arteries, and so on of the rest. In these complex organs, further, the origin of the components may vary; thus in the same subject, when the two eyes become fused, we have a complex organ, which may be called homogeneous, as the two components proceed from the same individual. Simple monsters then can only present homogeneous complex organs, on the contrary in double monsters, the eye for instance of one subject is united to the eye of the other, from whence we have a complex organ, which the author names heterogeneous, because each of its components has proceeded from a different individual, hence all single complex organs are heterogeneous, and can be only met with in double monsters.

But double monsters present also organs to which the author has given the name of simple heterogeneous, by which he means organs natural in appearance, but of which one half is furnished by one of the united subjects, the other half by the other.

The author adduces the anormal organs of monsters by excess to the two following conditions; 1st, double organs produced by the penetration of two organs which are simple in an ordinary infant; 2dly, double organs, apparently as simple as they exist in the normal state, but with this difference, that each infant furnishes half of the organ, so that the parts which appear simple in a double monster, are common to the two infants. Here we have a perfect unity produced by two distinct individualities; from this it follows, that there is no new single organ in these monsters, that is to say, there does not exist in them a single part formed with materials which are not found in an ordinary infant; now if there be no new organs, there is then no necessity for special laws for their development, if there be neither special laws, nor new organs, we cannot suppose that there are particular germs for these monsters, what purpose would they serve, since the formation is so well explained by the ordinary germs? Finally, if there are no new germs we see why and how it is that these monsters and their organs remain narrowly circumscribed in the limits of their species, their families, and classes.

We shall pass over a most profound and interesting analysis of the anatomy of double monsters; as we before said, a constant reference to the plates is necessary in order to follow the author.

The 16th section is devoted to the limits of monstrosity by excess. We have before alluded to the beautiful law, that developments may be arrested in their progress, so as to produce an organ inferior as compared with its analogous parts in be-

ings of the same scale, but we never observe an organ elevated to a superior rank in the organic scale. It is by this law that the harmony of organized beings is maintained, no matter what the organic exuberance of the monsters by excess may be, never do they pass their respective limits; monstrous fish remain fish, they can never be elevated to the organization of reptiles. Reptiles, monstrous by excess, do not become birds, birds cannot be elevated to quadrupeds, or those to man, all the productive force is exhausted in doubling the fish, the reptile, the bird, the quadruped, or the man, we have associated double individuals, and nothing more.

The author proceeds to shew, that from the mechanism of this double formation which he has demonstrated, these double parts can be only considered as the repetition, one of the other, and that the second part which repeats the first must be necessarily in its immediate vicinity.

“Suppose,” says our author, “a cephalo-dyme, composed of double materials, represented by the letters, A A', B B', it is evident, that the first head resulting from the reunion of the materials, A B, and the second from the reunion of A' B', these two orders of materials should necessarily remain beside one another. The materials, A B, could not be associated above the cervical region, while those of A' B' were united at the coccyx or elsewhere. The homologic system, which admits of such transpositions, supposes more than what is observed in nature, for if such a transposition was possible, even according to the homologic ideas, it would occur certainly in the extremities. We know from the time of Vic D'Azyr, that the superior and inferior extremities are analogous, that the composition of one is the repetition of that of the others. Now has it ever been seen in monsters, that the superior extremities took the place of the inferior, or vice versa? no certainly, nor will it ever be seen, such a transposition is incompatible with the laws of organic developments. If these parts, almost identical, cannot replace one another, how are we to suppose that the head shall be developed towards the sacrum, &c.? No hypothesis can give an appearance of truth to such a mutation. The organic transpositions are then as limited as the development of species and classes.”

This limitation of the association and position of organic materials becomes one of the powerful means which nature employs to render as little irregular as possible those creations which she forms with similar elements, her tendency to regularity is manifested even in the formation of irregular productions.

We now come to the examination of one of the most important chapters of the work, viz. that devoted to pathological anatomy, in which the author endeavours to shew the analogies between organogeny and pathological anatomy, and that they

reciprocally explain one another. Aberration in form producing monstrosity, that of structure gives rise to diseases; both according to Serres are subjected to almost the same rules. He compares organic diseases, on the one hand, to monstrosities by defect, on the other to monstrosities by excess: those differences in which we have degeneration (*reculement*) of tissues, correspond to the monstrosities by defect, while those in which a new organic product is formed correspond to the monstrosities by excess. The author ingeniously remarks, that as monstrosity by excess can only repeat an organization already acquired, so the productive diseases can only give birth to a tissue already existing; they reproduce tissues, as monstrosity reproduces organs, but these tissues, as well as these organs, have almost always their analogous formations in normal and regular organization. Thus we see in these new products, cellular, fibrous, osseous, or cartilaginous tissues, muscular fibres, and secreting organs, either in the form of cysts or in that of crypts, of which we see analogous instances already in the organization. We see further, blood-vessels spontaneously developed, and even a nervous system, which in certain cases is added to these accidental productions; and as in the formation of regular crystals, and in the intra-uterine developments, a slow action is indispensably necessary, so we find that those diseases in which new tissues are produced, are more or less chronic, while the more acute or rapid maladies produce the death of organs, hæmorrhages, or serous and purulent collections, in the interior of which no organic movement is sensible.

M. Serres endeavours to shew, that in the transformation of tissues, elements of different natures can never assume the same constitution. For instance, that the cellular element can only be converted into fibrous, serous, cartilaginous, or osseous tissue, but never into muscular or nervous matter, and thus draws the analogy between separate tissues, and the different organic developments of the zoological scale. He maintains further, adopting the scale of tissues of Bichat, that as in monsters, an inferior organization can never be elevated to a superior, while a superior may be degraded, by arrest of development, to an inferior, so in the separate tissues, assuming the cellular as the lowest, and the nervous as the highest in the scale, the same law will be found to hold good.

In speaking of Pathological Anatomy, the author properly remarks, that pathologists have erred in considering morbid changes as necessarily new states of the organization, an error arising from the comparison been drawn between the anormal state of the organ, and its normal state at the full period of development. Thus, when we speak of a part being indurated

or softened, it is only with relation to its state in health, at the full period of its development. But if at a certain period this part was *naturally* softened or indurated, we would have then another term of relation, and the change might be considered, not as a new state, but as a return to a former one.

As an illustration of this position, the following examples are given. Hydrocephalus, which, as far as the serous fluid is concerned, repeats the foetal condition: hydro-rachis also; here the accidental dropsy of the infant is the natural dropsy of the embryo. In the hydrocephalic child we find an increased volume of the head, a softened brain, and a congested pia mater, all corresponding to the increased dimensions of the foetal head, the consistence of its brain, and the relative development of its internal and external carotids. He next adduces the muscular system as a proof of his position, and appeals to the flabby anemic state of the muscles of old men, and also those of paralysed limbs. The aneurismatic state of the heart, the enlargement of the liver, and particularly of its left lobe, are farther instances of the repetition of the foetal condition of organs.

With respect to acute diseases, he remarks on the various alterations of the gastro-intestinal mucous membrane, particularly its softening, of which he traces the analogies with various periods of intra-uterine development, and the different results of sanguineous congestion of the lung and liver, which he maintains to exactly represent their foetal conditions at different periods, and concludes this chapter by the following remarks:

“ In conclusion, the structure of organs is modified by diseases as their form is modified by monstrosities.

These modifications cause either transformations in tissues and organs, or give rise to new organic productions.

“ New productions repeat tissues already existing, as the monstrosity by excess reproduces organizations already acquired.

“ Organic transformations, on the contrary, cause tissues and organs to return from the elevated condition in which their development had placed them, towards other states lower in the scale, but which were natural to them in the course of their intra-uterine life.

“ These two principal sources of organic diseases are then nothing but an unusual repetition of a process familiar to nature.”

We cannot help remarking, that the opinions maintained in this chapter, ingenious and interesting though they be, are yet far from being proved by the facts and arguments which the author has brought forward, and much more investigation and accuracy will be required, before the position of the author can

be considered as established ; it is also to be observed, that the essence of this doctrine is not original with M. Serres, as one of the fundamental propositions of the doctrine of Broussais is, that "diseased action has in it nothing new to the organization, but is reducible to a plus or minus degree of organic vitality." How many diseases are there which at present are irreducible to this formula ? We admit that an analogy (certainly, even on his own shewing, a loose one, inasmuch as he merely speaks of structure, independent of chemical composition) may be drawn between several cases of morbid action in the adult, and physiological action in the foetus ; but when he says that the diseases of organs are nothing but the return of the organization on itself, so as to represent their foetal condition, we are completely at issue with him. For instance, the obliteration of an artery has its analogies in intra-uterine development, but what foetal condition does the obliteration of the femoral artery for example represent ? Again, we see organs atrophied ; atrophy is a process foetal certainly, taken in a general sense, but the atrophy of certain organs is never a foetal process, and therefore in these cases the rule will not apply. How constantly, too, do we meet opposite states in the same organ, thickening in one place, thinning in another ; atrophy of one portion, hypertrophy of another, induration and softening, and so on ! Have these organs returned to their foetal condition ? or does one part of them represent one stage of embryonary life, and another part another ? The proposition of the author is divisible into two parts ; first, as connected with morbid action generally considered ; and, secondly, as connected with the actual changes of particular organs, and their analogies with their foetal condition. We may say safely, that what may be true in this doctrine, is not new, and what is new is far from being proved to be true. Even in the cases where the analogy of the change of organs may be observed at one stage of the process, as, for instance, in the soft solidification of the lung, the formula is totally inapplicable, when we consider the more advanced stages of the disease.

The remainder of the work is occupied with a resumé of the general etiology of organic deformities, and the connexion between the development of organs and the volume and direction of their arteries, and with a minute anatomical examination of the double monster, *Ritta Christina*.

In speaking of the inequality of development of the two sides of the body, the author reverts to the observations of Malpighi, Haller, Meckel, and Dutrochet, which shew that in birds the right umbilical artery, as compared with the left, is so little developed, that it seems, as it were, struck with an original atrophy ; this observation he declares to be verified with

respect to man, but maintains that the right umbilical artery corresponds to the left side of the aorta and the body, while the left artery corresponds to the formation of the right side, and from this he deduces the preponderance of the right over the left side of the body, not only in its muscular developments, but in all its organization. He thus explains the difference in organization between the sigmoid flexure of the colon and cæcum, the low organization of the spleen, as compared with the liver, the debility of the left lung and of the left hemisphere of the brain, curvatures of the spine with the convexity towards the right side, the frequent absence or deformity of the left kidney, and the more frequent stasis of the left testicle in the abdomen than the right. The arrest of development of the latter organ, in situation at least reproduces the ovary of the female; and Zacchias has already remarked, that in hermaphroditism the resemblance to the male is at the right, the resemblance to the female at the left side of the body; and the author remarks, that if, as Meckel, Blainville, and Geoffroy St. Hilaire, have stated, the production of the female sex is the result of a less energetic generative force, we can then see why it is, in the aberration of the male foetus that the feminine resemblances are found on the left side of the body. Further, if it is true that the relative feebleness of the left side predisposes it to monstrous formations, we can further see, why, in the statistics of monstrosity, the female sex should predominate over the male.

These views are highly ingenious, but we doubt much whether the conclusion drawn by the author, that diseased action preponderates at the left side of the body, is actually true. We rather believe that it will be found, that the organs on the right side of the body are more subject to inflammatory disease at least, than those on the left, a circumstance, in our opinion, perfectly consistent with their originally superior development. Authors have described apoplexies as more frequent on the right than the left hemisphere of the brain; the right lung is much more frequently the seat of pneumonia than the left, the liver more frequently the seat of inflammation than the spleen, the pylorus than the cardia, and the cæcum than the sigmoid flexure of the colon. If we assume the doctrine of the physiological school, that these diseases are the result of a plus degree of organic vitality, we can understand why the organs at the right side should be more subject than those at the left. An objection might be raised at first view to the doctrine of Serres, from the consideration of the fact, that the right half of the brain is the governor of the innervation of the left side of the body; yet it must be remembered, that the right hemisphere is developed by the same vessels as the rest of the organs on

that side of the body ; and here, perhaps, may be an explanation of the utility of the crossing of the cerebral fibres, as by a more vigorous innervation nature seeks to compensate for the deficient development of the left side of the body.

In concluding this analysis, we may repeat, that the work of Serres is a most profound and interesting one, and well worthy of the perusal of all those who aspire to the knowledge of the higher branches of medicine.

SCIENTIFIC INTELLIGENCE.

CHEMICAL AND PHYSICAL SCIENCE.

On the Mutual Action of Phosphoric Acid and Alcohol, by J. Pelouze.—The remarkable phenomena which accompany the action of sulphuric acid on alcohol, and the numerous products which result from this action, have been the object of investigations so profound, that this class of phenomena may now be compared to any thing which inorganic chemistry presents most exact and precise. However the different chemists, who have directed their attention to ether, having almost exclusively employed sulphuric acid in their experiments, it became important for the general history of etherification not to confine our attention to this single subject, and to examine with care how far the analogy extended of the action of the other minerals in their contact with alcohol.

The acids of this class, capable of converting alcohol into ether, are four in number, scil. sulphuric, phosphoric, arsenic, and fluoboric acids. The first attempts made with the view of transforming alcohol into ether, by means of phosphoric acid, were made unsuccessfully by Scheele, Lavoisier, and the academicians of Dijon. Some years later, Bondet, jun., a Paris apothecary, published in the 40th vol. of the *Annales de Chimie* a paper, in which he announced a real action between this acid and alcohol, and the development of most of the phenomena which accompany etherification; but the product he obtained being mixed with a considerable quantity of alcohol, prevented him from discovering its nature, and he did not attribute to it the characters of a true ether. At length, in the course of the year 1807, there appeared a very remarkable dissertation of M. Boulay, sen. on the ethers. This chemist proved incontestibly, that phosphoric and arsenic acids were capable of converting alcohol into an ether possessing all the properties of that obtained with sulphuric acid. Fourcroy and Vanquelin, commissioned by the Academy of Sciences to examine M. Boulay's work, confirmed all its results, and admitted, with him, a perfect identity between these two bodies. After the publication of the singular remarks of Dabit relative to a new acid, and to some new salts produced during etherification, the attention of chemists being eagerly directed towards this new series of phenomena, M. Lassaigne wished to ascertain, whether the phosphoric and arsenic acids did not furnish some compounds similar to sulphovinic acids, and to the sulphovicates.

He caused some phosphoric acid to act on alcohol, saturated the liquor with lime, then evaporated it, and obtained a salt, which by calcination yielded water, oil of wine, some gases with the odour of acetic ether, charcoal, and some phosphate of lime. The same salt, decomposed by the combined action of nitric acid and of heat, afforded to M. Lassaigne a white residue insoluble in water, and possessing all the properties of phosphate of lime. This chemist pushed his researches no farther, which were otherwise sufficient to assimilate, as he did, the action of phosphoric acid or alcohol to that of sulphuric acid. Besides, at the period when he undertook them, the remarkable works of Hennell and Serullas, and those equally important ones of Dumas and Boullay, jun., were not yet known; and it was not surely with a body converting alcohol into ether with so much difficulty as phosphoric does, that M. Lassaigne could expect to throw any light on the history of etherification, at that time so obscure. Such was the state of things when I undertook the matter which I have the honour of submitting to the Academy. I first began by repeating the experiment of M. Boullay, and I obtained, as he did, notable quantities of ether, by causing phosphoric and arsenic acids to re-act on alcohol. I then satisfied myself that a mixture of phosphoric acid and of alcohol, both in a concentrated state, shaken with an excess of water of baryta, boiled and filtered, produced a copious precipitate with sulphuric acid. I paid still further attention to this action, and I was fortunate enough to separate a new acid and new salts, which I shall call *phosphovinic acid* and *phosphovinates*, in order to conform to the nomenclature generally adopted for sulphovinic acid and the sulphovinates. The action of phosphoric acid on alcohol varies with the degree of concentration of these two bodies, their relative quantities, and the temperature to which the mixture is subjected. When the acid has a density equal to it, or below 1.2, it produces no sort of alteration on the alcohol, whatever the degree of concentration of this latter may be. When, on the contrary, the acid is very much concentrated, when its consistence is that of a very thick syrup, and when it is heated in this state with the fifth part of its weight of alcohol, a brisk effervescence takes place; there is produced a considerable quantity of per-carburetted hydrogen, of oil of wine; the liquor becomes very brown, and deposits some charcoal in the form of light black flakes. If the phosphoric acid and alcohol, both concentrated, are mixed in equal quantities, or nearly equal in weight, a thermometer placed in this mixture is soon raised to 176° Fahr. The liquor, subjected to a gentle ebullition, diffuses an ethereal odour very agreeable, remains perfectly colourless, and affords, when saturated with carbonate of baryta, a notable quantity of phosphovinate. There is then, as we may already perceive, a great similarity of action between the phosphoric and sulphuric acids in their contact with alcohol; for not only do these two acids convert it into ether, or not convert it, according as they are concentrated or weak, but moreover, the production of the ether is intimately connected in the two cases with that of a par-

ticular acid. I shall return to this latter point at the end of my paper, and I proceed directly to the preparation and properties of phosphovinic acid and the phosphovinates. We mix 100 grammes of alcohol of 95° (centesimal) and 100 grammes of phosphoric acid, the consistence of which must be that of a very thick syrup; we keep this mixture for some minutes at a temperature of from 140° to 176° Fahr.; at the end of twenty-four hours we dilute it with seven or eight times its volume of water; it is then neutralized with carbonate of baryta, reduced to an extremely fine powder. We then raise the liquor to a state of ebullition, in order to volatilize the excess of alcohol; it is then let cool down to 158°, and filtered. On cooling we obtain a very beautiful white salt, usually deposited in the form of hexagonal plates. This is phosphovinate of baryta. This salt, once obtained, may easily serve for the preparation of other phosphovinates, and of phosphovinic acid. Its properties are these: it is white, without odour, an agreeable taste, saline and bitter at the same time, like that of all the soluble barytic salts. Exposed to the contact of air, it effloresces, but very slowly; it is insoluble in alcohol and ether, which immediately precipitate it from its aqueous solution. Its solubility in water is remarkable in this particular, that it is not increased, like that of almost all other bodies, by temperature. It is at its *maximum* at about 104° Fahr. Above and below this it lets fall a precipitate. The following is the table of this solubility:—

100 parts of water dissolve of it at	-	0	3.40 parts
100 - - - - -	-	5°	3.30
100 - - - - -	-	20°	6.72
100 - - - - -	-	40°	9.36
100 - - - - -	-	50°	7.96
100 - - - - -	-	55°	8.87
100 - - - - -	-	60°	8.08
100 - - - - -	-	80°	4.49
100 - - - - -	-	100°	2.80

The salt precipitated at 100° from its aqueous solution is not anhydrous. It contains the same quantity of water as that which is crystallized at the ordinary temperature. The phosphovinate of baryta crystallizes in different forms, all, however, derived from a very short prism, the bases being an oblique angled parallelogram.

The angles could not be measured, the crystalline plates being too soft, and the sides not reflecting sufficient light for the purpose.

Submitted to the action of heat, the phosphovinate of baryta loses its water of crystallization, which constitutes three-tenths of its weight, and assumes the brilliant aspect of mother of pearl. It begins to be decomposed only at a little below a dark red heat, and then yields water, carburetted hydrogen gas, traces scarcely perceivable of alcohol and of ether, and a residue consisting of a neutral phosphate of baryta, and some charcoal in a very divided state. There is an entire absence of oil of wine, and of phosphuretted hy-

drogen. If the phosphovinate of baryta were a neutral salt, as the sulphovinate, it would yield by calcination either phosphorus or phosphuretted hydrogen gas, because then there would be established a re-action between the elements of the alcohol, and the excess of acid of the salt become bi-phosphate of baryta; but it is not so by reason of the basic nature of the salt, in which there is only a quantity of baryta exactly necessary to produce a neutral phosphate with the entire of the phosphoric acid. The water poured on the black residue does not take up any soluble matter; nor does it at all affect test paper, nor is there observed any disengagement of phosphuretted hydrogen, which proves the absence of phosphuret of barium in the residue. Nitric acid brought in contact in the cold state with phosphovinate of baryta renders it opaline. There is a formation of phosphovinic acid, and of nitrate of baryta, which may be easily separated by means of alcohol, in which this salt is insoluble. The same phosphovinate dried and heated with carbonate of potash does not yield alcohol, as the sulphovinate of the same base does, according to Wohler and Liébig. The mixture is not destroyed, and does not commence to become black till a little before the dark red heat, without the presence of the carbonate of potash entering for any thing into the phenomenon. The following salts, chloride of manganese, proto-chloride and per-chloride of iron, chloride of nickel, chloride of platina, chloride of copper, or of gold, are not precipitated from their aqueous solution by the phosphovinate of baryta; but it forms, on the contrary, precipitates more or less abundant in the salts of tin at the *minimum*, in the salts of mercury, of silver, of lead, and of lime. All the phosphovinates thus prepared by double decomposition are dissolved in diluted acids. Those which are soluble, such as those of potash, soda, ammonia, and magnesia, are very easily obtained by decomposing by the phosphovinate of baryta the sulphates of these different bases.

The phosphovinate of potassa crystallizes with much difficulty, and in a manner too much confused for it to be possible for me to determine the form it assumes. It is very deliquescent, and fusible in its water of crystallization. The same may be said concerning that of soda. That of lime contains four atoms of water of crystallization; it is very little soluble, and is precipitated in the form of small plates extremely brilliant, when phosphovinate of baryta is poured into nitrate or muriate of lime. It is readily dissolved in water acidulated with vinegar, or with phosphovinic acid.

The phosphovinate of strontian crystallizes with difficulty. Like to that of baryta, it is much less soluble in boiling water than in warm water. It contains some water of crystallization, the quantity of which is not determined. Alcohol precipitates it from its watery solution. The phosphovinate of silver resembles as to its aspect and its sparing solubility that of lime, and is readily obtained by double decomposition with nitrate of silver and phosphovinate of baryta. It contains some water of crystallization. That of lead is the most insoluble of all, and is precipitated in the anhydrous state. Of all these salts I

have subjected but two to analysis, scil. that of baryta, and that of lead.

5.908 of crystallized phosphovinate of baryta, dried at 248° Fahr., have been reduced to 4.126; 1.775 lost, in the second experiment, 0.550 of water.

The mean gives—

30.575	water.
69.425	dry salt.
<hr/>	
100.000	

5.000 of the dry salt, decomposed by nitric acid, and calcined at a red heat in a platina crucible, yielded 4.140 of phosphate of baryta. Again, five other grammes of the same phosphovinate dried, dissolved in water, and precipitated by sulphuric acid, gave 4.308 of sulphate of baryta. The numbers 4.140 and 4.308 being to each other, even to the last decimal, as the weight of an atom of neutral phosphate of baryta is to that of two atoms of the sulphate of the same base, it hence follows, that in the phosphovinate of baryta the phosphoric acid and the baryta are in the exact proportion to produce a neutral phosphate. The analysis of the organic substance of the salt was made with the excellent apparatus of Dr. Liébig. Its execution is infinitely more easy than that of the sulphovinate of baryta, which is mixed with the acid of copper, forms a paste with it, and always yields, proceed as we may, some sulphurous acid. I have repeated these analyses several times, and always obtained satisfactory results.

	Salt.		A. Carbonic.		Water.
1°. 1.956	-	0.639	-	0.390	
2°. 3.000	-	1.002	-	0.719	
3°. 3.244	-	1.085	-	0.673	

The mean of 9.166 of carbon and 2.266 of hydrogen for 100 of phosphovinate.

This salt is then formed of—

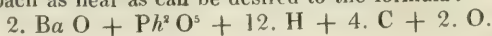
82.800	neutral phosphate of baryta.
9.166	carbon.
2.266	hydrogen.
5.768	oxygen.

100.000

These numbers translated into atoms give—

2.	Ba O.
	Ph ² O ⁵ .
	H ¹² , 35.
	C ⁴ , 06.
	O ¹ , 95.

and approach as near as can be desired to the formula:—



($H^1 C^2 + H^2 O$) representing an atom of alcohol, the phosphovinate of baryta may be represented as a sesquibasic salt, in which an atom of phosphoric acid will be saturated by two atoms of baryta, and by two atoms of alcohol. With respect to its water of crystallization, it contains twelve atoms, according to the two experiments mentioned above. The dry phosphovinate of baryta may be represented in the following formula: $2. Ba O + 2 (H^1 C^2 + H^2 O) + Ph^2 O^5$, and the same salt hydrated $= 2. Ba O + 2 (H^1 C^2 + H^2 O) + Ph^2 O^5 + 12. H^2 O$.

The analysis of the phosphovinate of lead was performed by the same processes.

5 grs. gave neutral phosphate of lead $= 4.314$.

1° 4 grs. gave carbon. acid $= 1,135$. water $= 0.655$.

2° 4 grs. gave carbon. acid $= 1,118$. water $= 0.648$.

These experiments lead to the following composition:—

2 atoms of neutral phosphate of lead.

2 atoms of alcohol.

1 atom of phosphoric acid.

Its formula is, $2. Ph O + 2 (H^1 C^2 + H^2 O) + Ph^2 O^5$.

Thus the phosphovinates are double salts, in which two atoms of inorganic base, and two atoms of alcohol, are combined with one atom of phosphoric acid. Two atoms of alcohol being equivalent to one atom of an inorganic oxide, the phosphoric acid must require double the quantity of it, i. e. four atoms to form a neutral salt; whence it follows, that supposing that the alcohol enters as a base into the phosphovinates, and performs the part of an electro-positive body, these salts must be considered as sesquibasic, and the acid which they contain as a biphosphate of alcohol, of which the formula is, $2 (H^1 C^2 + H^2 O) + Ph^2 O^5$. This acid is obtained by a process similar to that for sulphovinic acid. The phosphovinate of baryta is dissolved in water, and dilute sulphuric acid is poured on by little and little, till a precipitate ceases to be produced. We filter and evaporate the liquor first at the open fire, then in vacuo, by the side of a vessel filled with sulphuric acid. We obtain a liquid, which, being brought to the consistence of a thick oil, refuses further concentration, but still is not decomposed at the ordinary temperature in the vacuum, as is the case with sulphovinic acid. This acid may be equally well procured by decomposing the phosphovinate of lead by sulphuretted hydrogen. It is a body of a pungent and very acid taste, without odour, without colour, of an oily consistence, reddening very deeply the blue colour of turnesol, soluble in all proportions in water, alcohol, and ether, capable of perfectly resisting long continued ebullition, when it is dissolved in several times its volume of water, decomposed on the contrary at this same temperature, when it is at its *maximum* of concentration, yielding at first a mixture of ether and alcohol, then of carburetted hydrogen, some traces of oil of wine, and a residue of phosphoric acid mixed with carbon. I was not able to obtain in the solid state a sufficient quantity of phospho-

vinic acid to submit it to analysis. There is some however formed in that, which is very concentrated, and we see it precipitated in the form of small crystals very brilliant in the sun's light: a cold of -22° (centigr.) does not augment their number. Phosphovinic acid coagulates albumen, when either it arises from the re-action of the ordinary phosphovinic acid on alcohol, or may have been produced by phosphoric acid previously calcined at a red heat. I have not observed the slightest difference either in the properties, or in the composition of the phosphovinic acid, and phosphovinates prepared with the phosphoric and paraphosphoric acids. The phosphoric acid derived from the salts, which I at first thought to be paraphosphovinates, being neutralized by potash and treated with nitrate of silver, constantly yielded a yellow precipitate, which inclines me to think that the paraphosphoric acid is not capable of forming double salts with the inorganic bases and alcohol, and that in acting on this latter body its isomeric property leaves it. However, the thing is too delicate for me to venture to regard this opinion as the expression of truth. The phosphovinic acid diluted with water, and brought in contact in the cold with zinc and iron, gives rise to a copious disengagement of hydrogen gas, and to the formation of a phosphovinate of the protoxide of iron or zinc. It disengages the carbonic acid from all the carbonates, and forms with their oxides, salts, the greater part of which are soluble. The sulphuric acid and water of baryta do not disturb its aqueous solution. Curious to know if the phosphovinic acid is produced in as large quantities in the cold as at a high temperature, and how much there is formed of it for a given weight of phosphoric acid, I instituted the following experiments, the idea of which was suggested to me by reading the excellent memoir of Hennell on sulphuric ether.

Ten grammes of very concentrated phosphoric acid were dissolved in water; 10 grammes more were dissolved in an equal weight of alcohol at 95° (centigr.), and the mixture was left to itself in an ice bath for 24 hours; 10 grammes of the same acid were subjected for some minutes to ebullition with their weight of the same alcohol.

The first liquor yielded 21.8 of phosphate of baryta.

The second - - 15.0

The third - - 14.8

These experiments prove, that in re-acting on alcohol, about the one-fourth of the phosphoric acid employed is converted into phosphovinic acid, and that this transformation, which takes place in the cold, is not sensibly modified by the ebullition of the mixture. They equally prove, that the decomposition of the phosphovinic acid is much more difficult to be effected than that of the sulphovinic acid. It is to this circumstance we must refer the very scanty production of ether, when we cause phosphoric acid to re-act on alcohol, and not, as was supposed, to the inaction of phosphoric acid on this liquid, since even at the temperature of melting ice, the contact of these two bodies determines the formation of a considerable quantity of phosphoric acid.

Some of the facts stated in this paper ought, if I am not mistaken, to bring some modification in the theory of the formation of ether, such as has been given by Hennell and Serullas. They are equally opposed, at least in some points, with the otherwise ingenious views of Dumas and Boulay, jun. with respect to the part which olefiant gas performs, in the compounds of which it constitutes a part.

According to these two last chemists, the bi-carbonated hydrogen gas should be considered as really possessing the alkaline character, and its combinations should be assimilated to those of ammonia, with which there is, as I may say, a perfect analogy. In their paper* on the subject they express themselves thus: "The salt, which we obtained on treating oxalic ether with ammonia, contains two volumes of ammonia and two volumes of bi-carbonated hydrogen gas, which would be necessary to complete the neutral oxalate of ammonia. The bi-carbonated hydrogen has then the same capacity of saturation as ammonia." "In hydrochloric and hydriotic ether one volume of acid gas is saturated by one volume of bi-carbonated hydrogen gas, in the same manner as in the neutral hydrochlorate and hydriodate of ammonia, the acid and base are combined volume to volume. The capacity of saturation is the same here also." "One atom of the hyponitrous, acetic, benzoic, and oxalic acids, saturates four volumes of ammonia: now in the ethers formed by these acids the atom of each also exactly saturates four volumes of per-carbonated hydrogen. The capacity of saturation is still the same in this case." "Finally, in the sulphovinates one atom of sulphuric acid is exactly saturated by four volumes of per-carbonated hydrogen, as it would be by four volumes of ammonia." "The ammonia and carbonated hydrogen, in combining with the hydracids, form anhydrous combinations, and with the oxacids, hydrated salts, or hydrated ethers, that is to say, combinations of bi-carbonated hydrogen, acid, and water, in which the saturation of the acid is complete." A discussion so profound, and so rigorously deduced from a great number of facts, could not fail to gain the conviction of chemists, for never was a more perfect analogy demonstrated between any two bodies. But four years after the publication of the memoir of Dumas and Boulay, there appeared an analysis of the sulphovinate of baryta by Messrs. Wohler and Liebig,† an analysis, the result of which led the authors to consider this salt, not as a double sulphate of baryta and of alcohol, the formula of which is: $2.503 + BaO + 2(H^1C^2 + H^2O)$; wherefore the sulphovinic acid must be $2.503 + 2(H^1C^2 + H^2O)$, that is to say, a bi-sulphate of alcohol. However, the extreme facility with which the sulphovinates are decomposed, the uncertainty which must always remain over their state of desiccation, did not yet warrant an opinion to be irrevocably established with respect to the true nature of these salts, and the part which their acid performs in the phenomena of etherification.

* *Annales de Chimie et de Physique*, tome xxxvii. p. 42.

† *Ibid.* tome xlvii.

The great permanency of the phosphovinates, the high temperature to which they may be submitted without decomposition, (they perfectly resist a temperature which would burn wood, starch, or tartaric acid,) do not permit us to raise the least doubt with regard to their perfect desiccation; and as the analysis always presents the per-carbonated hydrogen gas and water in the proportions, which exactly constitute alcohol as one of them, that of baryta, heated to upwards of two hundred degrees (centigr.) in vacuo, loses none of its weight, and yields, even after the application of this elevated temperature, the elements of alcohol, it becomes extremely probable, not to say certain, that the alcohol exists completely formed in these salts.

If it be so, the analogy between ammoniacal gas and olefiant gas ceases to be as complete as it appeared at first, since to preserve the analogy, it would be necessary to admit, a thing not very likely, that the same base, bi-carbonated hydrogen, saturates the acids in three different ways; sometimes in the anhydrous state, as in the ethers formed by the hydracids; sometimes with the proportion of water which constitutes sulphuric ether, as in the vegetable ethers; sometimes, in fine, with the double of this water in the sulphovinates and phosphovinates. However, the observation of Dumas and Boulay, that an atom of sulphuric acid, and in general the atom of any acid whatever (except the phosphoric and arsenic acids, whose saturation follows a different law from that of the other acids) is exactly neutralized by four volumes of olefiant gas, and by four volumes of ammoniacal gas: this observation, which is that to which the chemists I have cited have attached most importance, far from being invalidated by the analysis of the phosphovinates, has acquired, on the contrary, an additional degree of certainty. Be it as it may, with respect to the true nature of the phosphovinates, and definitively speaking, every thing that can be said on that head, is a mere play on words; the only thing incontestible is, that a phosphovinate is composed of a metal of phosphorus, of oxygen, of hydrogen, and of carbon, in the proportion which constitute a neutral phosphate, and of alcohol. It naturally follows that the theory of etherification, such as it is admitted at present, cannot be sustained without considerable modification. This theory consists, as we know, in admitting that the action of the sulphuric acid on alcohol gives rise to a compound of this body and of the elements of ether, and that by boiling the ether quits the acid with which it was combined, becomes free, and is disengaged. But if, on one side, the sulphovinic acid is a bi-sulphate of alcohol, according to the analysis of Messrs. Liébig and Wohler; if, on the other side, the phosphovinic acid has really the composition which I have found it to have, it is evident that these two acids, before producing ether, must undergo such a decomposition as that half the water of the alcohol which they contain goes to the sulphuric acid or to the phosphoric acid; whilst thus deprived of this water, the alcohol passes into the state of ether.

If phosphoric acid is less effectual in the formation of ether than

the sulphuric acid, it is not that it combines with more difficulty than the latter with alcohol, for we have seen the combination effected even in the cold; but it is owing to this that the phosphovinic acid resists decomposition much more than sulphovinic acid.

It might be objected to this theory, that the contact of sulphuric acid with ether gives rise to sulphovinic acid; but on reflection we might soon discover the little foundation for this objection, and the facility with which it might be removed. The vegetable ethers are compounds of sulphuric ether and of acids, as the experiments of Dumas and Boulay clearly shew. When we bring them in contact with alcalies, we obtain no ether, but a considerable quantity of alcohol.

It must be admitted that the same circumstance takes place here, that is, that the acid gives up its water to the ether to produce alcohol, which then combines with the sulphuric acid or with the phosphoric acid, for that occurs equally with the latter. In support of this mode of considering the matter, I may instance the much greater difficulty experienced in obtaining the sulphovinic and phosphovinic acids with ether than with alcohol; no doubt because in the latter case, the combination being capable of taking place immediately, there are no preventives to be encountered, whilst with ether there is a force to be overcome. The inverse of this should take place if the ether constituted part of these acids. To recapitulate, the experiments contained in this paper lead to the following conclusions:—

1°. The phosphoric acid in re-acting on alcohol, gives rise to a new compound, phosphovinic acid, formed of one atom of phosphoric acid, and of two atoms of alcohol.

2°. This acid forms with the different oxides very permanent salts, which should be considered as sesqui-basic phosphates, into which the alcohol enters as a constituent part, and which are formed of an atom of the neutral metallic phosphate, and of two atoms of alcohol.

3°. The sulphuric and phosphoric acids in contact with alcohol combine directly with it, form a bi-sulphate or a bi-phosphate of alcohol, which, subjected to the action of heat, are decomposed in water, in sulphuric, or phosphoric acid, and in ether.—*Journal de Chimie*, March, 1833.

Letter of M. Berzelius to MM. Wöhler and Liébig on Benzoyle and Benzoic Acid.

Stockholm, 2nd Sept. 1832.

“ In conformity with the desire which you have expressed to me, I have submitted to a revision my former researches on the composition of benzoic acid, and it has confirmed the result of your analysis in the most satisfactory manner. I have made, at your request, an analysis of benzoate of silver, and by a combustion carefully conducted I obtained from 100 parts of the salt of silver previously dried at 100°, 46,83 of metallic silver, a number which accords as exactly as can be required, with the theoretical result,

(46,86) which you have calculated. You have remarked, that my analysis of the benzoate of lead, as it is found in my former paper, perfectly coincides with the new composition which you have given. A recent analysis which I made with alcohol and sulphuric acid gave the same result, and confirms the presence of an atom of water of crystallization which I found in my first analysis. I here communicate to you the result of an analysis of benzoic acid, which I made in 1813, with the benzoic acid of sublimation, by burning in a tube the substance mixed with some chloride of calcium, and chlorate of potash.

“ 0,335 grammes of water gave, in this way, 0,138 gr. of water, and 0,855 of carbonic acid. On calculating 100 parts, we get :

Carbon,	68,85
Hydrogen,	4,99
Oxygen,	26,66

“ These numbers correspond exactly with the composition of the hydrated acid $C^{11}H^{12}O^4$. Having in vain attempted to separate from the crystallized benzoic acid, the water which it contained by saturating it with a weighed quantity of oxide of lead, and not being able to prove therein the presence of water of crystallization, whilst the result of this analysis afterwards gave 4 atoms of oxygen (though I had previously found by the analysis of the basic salt of lead, that the acid there saturates three times as much of acid as in the neutral benzoate of lead,) I determined, on seeing that these results could not be made to agree, to reject the analysis of the crystallized acid.

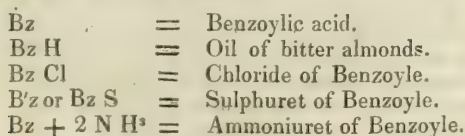
“ I then burned defined quantities of neutral benzoate of lead after having tried to deprive this salt of its water of crystallization by previously melting it. Each quantity which I analysed was prepared separately. Such has always been my principle, because in making but a single preparation, we may have the same fault in all the analyses. For this reason I have always melted each portion for analysis, and I always obtained variable results. I thought that these deviations must be attributed to a volatilization of undecomposed benzoic acid. On comparing however the results of these analyses, it is evident that the melted salts contained different residues of water. “ To prevent the volatilization of the acid, I employed the basic salt of lead: this is the analysis which I described. On calculating the result according to the rectified atomic weights, and comparing it with yours, here is what was obtained :

Result of the Former.		Result of the exact Analysis.	
Carbon,	75,405		74,703
Hydrogen,	4,951		4,356
Oxygen,	19,644		20,941

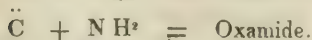
The former analysis differs then from the theoretical composition by 0,7 of carbon, and 0,595 of hydrogen: this excess diminish-

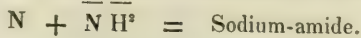
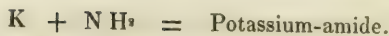
es so far the quantity of oxygen. The results which you have deduced from your researches on the oil of bitter almonds, are assuredly the most important as yet arrived at in organic chemistry; they promise to throw an altogether new light on this part of the science. Let a body composed of carbon, hydrogen, and oxygen, combine, after the manner of a simple body, with others, but particularly with those which form acids and bases, that is a fact which decides that there exist ternary compound atoms, (of the first order,) and that the radical of the benzoic acid is the first example which can be adduced with certainty of a ternary body possessing the properties of a simple body. True it is, we have already regarded sulpho-cyanogen as such; but you know that its combinations might also be regarded as sulphosalts, and even this body appears to be a sulphuret of cyanogen.

The facts adduced by you give rise to considerations of such a nature, that one might regard them as the commencement of a new era in organic chemistry. It is in this point of view that I proposed to you to give the name of *proïne* to this first radical, compounded of more than two bodies. It is borrowed from the word *πρωι*, *day break*, (*point du jour*;) or else we might call it *orthrine*, from the word *ὀρθρῆς*, which means *dawn of day*, (*aube du jour*.) With these denominations, we might form the names of the acids *proïnic* and *orthrinic*, the chloride of *proïne*, or of *orthrine*, &c. But on considering that the name of benzoic acid, so long in use, must also be changed, and that we are habituated to regard the ancient names as long as they do not present a double sense, inasmuch as we derive from them the new names, as for example we have formed *bore* from *boric acid*; *potassium* from *potassa*, &c.; for these reasons, I say, it appears to me preferable in every respect to adopt the name of *benzoyle* which you proposed, and to change the name of benzoic acid into that of *benzoylic acid*, so that we shall henceforward say *boric acid* instead of *boracic acid*. When once we know with any certainty ternary atoms of the first order which enter into combinations in the manner of simple bodies, it becomes very easy to give a sign to each radical to express it in the language of a formula. Thus the reader forms a precise and clear idea of the composition we wish to give. I shall give some examples of this. We put benzoyle $C^{14} H^{10} O^2 = Bz$; we shall then have:

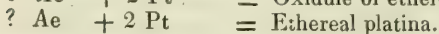
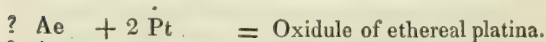
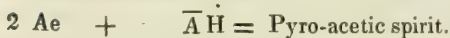
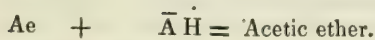
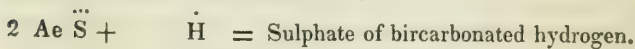
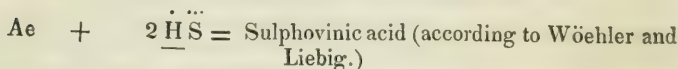
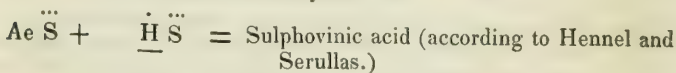
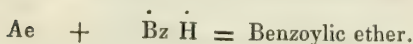
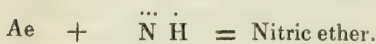


“ Putting *Amide* $= \underline{N H^2}$, we shall have:

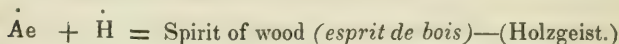




Let us represent the oil of wine which I purpose to call *etherine*, by $\text{C}^{\text{H}} \text{H}^{\text{H}} = \text{Ae}$, we shall have :



Let us suppose that there exists an oxide of etherine $= \text{Ae}$, we shall have :



According to these two formulæ, we see that acetal is to the spirit of wood what pyroacetic spirit is relatively to acetic ether.

A matter which I deem it right to insist is, that we should make use of formulæ only when the ideas which they should express rest on established truths, otherwise they would lead to extreme confusion.—*Annal. de Chimie*, November, 1832.

Chemical Action of Induced Electricity, Decomposition of Water, by M. Hacket.—We read in a paper of M. Faraday's, of the 24th November, 1831, that this gentleman had in vain tried to produce chemical effects by electric currents of induction. However he thought that such might be obtained by means of magnets more powerful than what he had used, and he saw by new researches, that

the difference at first noticed between the effects of ordinary electric currents and those produced by induction might altogether disappear. This opinion, distinctly expressed in the 57th and 59th articles of his paper, is completely verified by the following experiment: M. Pixii mounted a horse-shoe magnet, (*sur le bout de l'arbre d'un tour en l'air,*) and by means of a pedal he caused this magnet to revolve before a bar of soft iron, bent into the form of a horse-shoe; this was enveloped in a copper wire covered with silk. The two extremities of this wire were made to communicate with two other metallic wires which passed through the bottom of a vessel full of water. Each of these latter wires arose in a glass tube of the form of a small bell inverted. The water contained in the vessel and in the two tubes formed but one single liquid mass. Whilst the magnet revolved, it acts by induction on the magnetized soft iron, on the copper wire covered with silk, and on the two wires placed in the glass tubes. The decomposition of the water is effected at the extremities of the latter wires, the two gases, oxygen and hydrogen, ascend to the summit of each tube. It follows from this experiment; 1st, that it is not necessary, as was supposed, that the action of the two electricities positive and negative, should be simultaneous for the chemical decomposition of water; 2nd, that the action whose interruption is but instantaneous, may also produce this decomposition. These conclusions agree with the observations previously made regarding the decomposition of water by means of the Voltaic pile. This decomposition takes place, though the humid or liquid substances of the pile differ in their conducting power; we may conceive then that the water opposes a force of inertia to the electric action tending to decompose it, and that to overcome this inertia, it is necessary that an electric current should act on the water a certain time before this liquid is decomposed. The electric currents of induction seem to act as the continued electric currents of a Voltaic pile, where metallic plates should be separated by an imperfectly conducting liquid. I have shewn that in the case where these plates are separated by layers of starch slightly moistened, there are obtained dry piles of long duration which charge the condenser, and do not decompose water; though the electric current of these piles be continued, its velocity is then too much diminished to obtain chemical action, such as the decomposition of water. The magnet employed by M. Pixii for the decomposition of water is formed of two other horse-shoe magnets joined. Each of these separately raises $12\frac{1}{2}$ kilogrammes, and they weigh together 4 kilogrammes. The axle-tree (*l'arbre du tour*) made at least ten revolutions every second. The decomposition of the water increases with the velocity of rotation of the magnet. The section of the bar of soft iron on which the copper wire, covered with silk, was rolled, is a circle 4 centimeters in diameter; its height is 20 centimetres, it is bent into the horse-shoe form with parallel branches. The interval between the branches is eleven centimetres, reckoned from centre to centre on the circular

ends. The copper wire covered with silk is 400 metres in length, and weighs 2 kilogrammes.—*Annal. de Chim. et de Physiq.*, Sept.

Note of M. Ampère on the preceding.—M. Hachette has communicated to the Academy some experiments performed with an apparatus constructed by M. Pixii, to produce an electric current by turning a horse-shoe magnet before another of soft iron, around which a conducting wire, covered with silk, was twisted into a helix. After having obtained bright sparks with an apparatus, whose magnet raised thirty livres, and whose wire made 500 turns, by means of another whose magnet raised more than 100 kilogrammes, and whose wire is of the length of 1000 metres, and makes 4000 turn, there have been obtained, 1°, bright sparks; 2°, considerable shocks; 3°, when the hands are plunged into vessels full of acidulated water, into which the two extremities of the conducting wire enter, numbness and involuntary motion of the fingers; 4°, a great divergence of the gold leaves adapted to the condenser of Volta; 5°, a considerably rapid decomposition of water, mixed with a little sulphuric acid to increase its conducting powers.

In these different experiments the current took place in the conducting wire, in a direction different at each turn of the magnet, so that in the case of the decomposition of the water, the oxygen disengaging itself at first in one of the bells, and the hydrogen in the other, at the ensuing half turn, it was on the contrary the hydrogen which was disengaged in the first, and the oxygen in the second. Thus in each bell there was but a mixture of the two gases; to obtain them separately M. Hippolyte Pixii conceived the happy idea of applying to this apparatus the *bascule*, devised by M. Ampère, to change the current in his electro-dynamic experiments.

The *bascule* adapted to the new apparatus carries a stock, on which is supported a semi-circle attached to the magnet, and which holds the *bascule* depressed on one side during a semi-revolution of the magnet, whilst during the following semi-revolution the *bascule* becomes free, and is depressed on the other side by a spring. In the first trial of this arrangement the *bascule* plunged alternately into the troughs full of mercury, as into the *bascules* of M. Ampère; but when the motion became rapid, the mercury was so violently shaken that it was thrown from the troughs.

M. H. Pixii has obviated this inconvenience by replacing the mercury by small plates of copper amalgamated on their surface, to render more intimate their contact with the points of the *bascules* which strike them alternately. By means of this ingenious arrangement, the electric current in the part of the conducting wire beyond the *bascule* always takes place in the same direction, whence it follows that there is but oxygen disengaged into one of the bells, and hydrogen into the other, and thus the two gases are obtained separately. It may be remarked, that, *cæteris paribus*, the decomposition of the water becomes more rapid in this case than in that where the electric current alternates, owing probably to this, that the particles of the water are predisposed as they should be for decomposition, whilst when the current alternates, they must return at each semi-revolution of

the magnet. As to the phenomena, such as the sparks, shocks, action on the gold-leaf electroscope, there is no perceivable difference, whether we employ the current which always takes place in the same direction, or the alternating current, because all these phenomena result from the instantaneous action of the electricity developed in the conducting wire, an action sufficient to charge the condenser of the electrometer, as much as the tension of the current permits it.—*Ibid.*

Electro-Magnetic Machine.—M. Hachette has informed the Academy, (September sitting of 1832,) that an apparatus giving a continued series of electric sparks by means of a moveable needle has been recently constructed by the sons of M. Pixii. This apparatus consists of two horse-shoes of equal divergence, the one magnetized steel, the other soft iron. They are placed end to end in a vertical plane, which passes through their common axis; the opposed extremities, though very near, do not touch. The transverse sections are for the magnet a rectangle, and for the soft iron a circle, whose diameter might equal the width of the rectangle. A copper wire surrounded with silk makes several revolutions on the two branches of the soft iron; it is retained there by four circular plates of copper, of which two level the circular extremities of these branches. The extremities of the copper wire are directed towards a capsule, or glass bottle, containing mercury, and kept in a state of rest at a small distance from the surface of this metal. The horse-shoe magnet, whose curve is downwards, turns on its axis, which is vertical, and at each semi-revolution its two polar extremities, north and south, are in conjunction with the soft iron which are fixed, and on which the magnet acts by influence at the small distance which separates them. The same magnetary influence is communicated to the covered wire, and produces at the extremities of this wire a series of electric sparks perceivable on the surface of the mercury. If one of the ends of the copper wire surrounding the soft iron is immersed in the mercury, the spark will be brighter at the extremity not immersed, and two successive sparks will be owing to contrary electricities, as has been shewn in the excellent paper of M. Farady, published in these *Annals*, in the No. for May, 1832.

The soft iron and copper wire surrounded with silk which encompasses it being in a fixed position, the magnet is made to turn by means of a handle and wheel which catch a horizontal pinion mounted on the axis of rotation of the magnetized horse-shoe, which axis is beneath the curve of this latter. This new construction is founded on the well known fact that a magnet acts at a distance on soft iron, which becomes itself magnetized, and preserves its magnetism as long as this soft iron is within the sphere of the magnet's action.

The magnet in the apparatus constructed by MM. Pixii, weighs 2 kilogrammes and may raise 15. Its transverse section is a rectangle,

whose sides are 35 and 10 millimetres. The vertical height of the horse-shoe in the direction of the axis is 21 centimeters: the divergence of the polar extremities, 2 centimeters. This divergence is the same between the opposite ends of the soft iron, whose cylindrical branches are 15 millimetres in diameter. The heights of these branches is about 8 centimeters. The length of the wire is 50 metres, its weight a quarter of a kilogramme.

The glass bottle containing the mercury is placed in the wooden frame of the apparatus, and participates in the vibratory motion which arises from the shock of the teeth of the pinion fixed on the axis of the magnet by the wheel which catch this pinion.

This motion raises the surface of the mercury to the height of the extremity of the wire not immersed. The motion of the mercury taking another direction, the spark appears just where the extremity of the wire emerges from it.—*Annal. de Chim.*, Juillet, 1832.

BOTANY AND NATURAL HISTORY.

Respiration of Insects.—M. Dutrochet read a paper before the Royal Academy of the Sciences entitled: “On the Mechanism of the Respiration of Insects.” This function is always performed by means of tracheæ, which carry the respirable air through all parts of the body. This is observed in aerial insects as well as in those which inhabit the water. But it is easily conceived, that inhabiting these two mediums brings a marked difference in the mechanism by which the respirable air is introduced into the tracheæ. In aerial insects the air enters the tracheæ and goes from them by means of a muscular action which may be compared to that which takes place in deglutition. For aquatic insects, they sometimes draw in air from the atmosphere, by coming to respire at the surface of the water, sometimes they draw it in from the water by means of an apparatus called *gills*, though they differ essentially from the same organs in fishes; for the gills of the insects are organs which receive the water charged with respirable air, and extract from it this air to carry it by the tracheæ, to all parts of the body. By what mechanism does the air dissolved in the water pass again to the elastic state? Such is the question which M. Dutrochet has proposed to himself to solve in his paper. He supposed that he arrived at the solution by studying the reciprocal action of aerated water, and of the different gases, which the tracheæ of insects contain. The author instances the experiments made on this point by MM. Gay Lussac and De Humboldt, and states in detail those which he undertook to complete these researches. The results to which he has arrived are these. Every time that any mixture of azote, oxygen, and carbonic acid, enclosed in a cavity whose parietes are permeable, is placed in the midst of water

holding air in solution, there is, through the parietes of this envelope, a passage of the gases from the interior to the exterior, and reciprocally, a passage which is only arrested when the cavity contains no more of oxygen and azote than correspond to the proportions constituting atmospheric air.

These facts being well established, the author applies them to the theory of the respiration of those aquatic insects which respire in water. All are provided with gills, which being placed very superficially, permit the gases contained in their cavity to communicate with those dissolved in the water. The exchange may then take place through their parietes, as through those of the bladder, which M. Dutrochet used in his experiments, and, *cæteris paribus*, it ought even take place more rapidly, since here the form in tubes, brings it to pass, that for the same volume there is much more surface, and consequently a much greater number of points for the passage to take place.

Let us add, that the instinctive movements of the insect renew incessantly the contact of the aerated water in the gills, so that the apparatus is just as if it were placed in running water, a condition which is the most advantageous for the transformation of the interior gases into atmospheric air. Now what gas must there be in the gills? The same as in the rest of the tracheæ, that is to say, air deprived in part of oxygen, and charged with a portion of carbonic acid. Now such a mixture contained in vessels with their parietes immersed in aerated water, which are incessantly agitated therein, and which present, with respect to their volume, a very considerable surface, such a mixture, we say, ought, according to the laws previously observed, undergo a transformation, the end of which is to change it into respirable air.

Thus at the same time that in the tracheæ of the body, by a necessary effect of respiration, the air is deprived of its oxygen, and is charged with carbonic acid, in the tracheæ of the gills it yields to the water, the carbonic acid which in time rendered it irrespirable, the part of the azote which may be superabundant, and takes from the surrounding liquid a quantity of oxygen equal to that which it has lost.

It is not merely in the gills of aquatic insects that this mechanism may be observed. The following fact, for the first notice of which we are indebted to Reaumur, presents a curious example of it: on the submersed leaves of the *potamogeton lucens* a caterpillar lives which passes the entire time of its life of larva and chrysalis entirely under water, and yet, as its organization is adapted to live in air, it is necessary that it should be constantly surrounded by this fluid, and kept sheltered from the water, in which it must be drowned. It forms for itself then a shell of silk, protected on the outside by particles of the leaves of the *potamogeton*: this shell is open, and its interior contains air, in the midst of which the larva lives. When it is metamorphosed into a nymph, it completely shuts its shell, which continues to enclose air. Though constantly submersed then, the animal lives in

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air, and this air ceases not to be fit for respiration, though it experiences no apparent renewal. We see that in this case the permeable parietes of the shell of silk, must, as those of the bladder in the experiments of the author, suffer the necessary portion of oxygen to pass from without inwards, at the same time that they permit the egress of the carbonic acid gas, and of the excess of azote.—*Archives Generales*, Feb. 1833.

Vomiting in Ruminant Animals.—M. Flourens read a paper entitled, “Experiments regarding the Action of Tartar Emetic on Ruminant Animals.” In a preceding memoir the author established, by means of numerous experiments, that the vomiting proper to ruminant animals differs essentially from the vomiting of other animals in this, that instead of being, as the latter, a confused rejection in a mass, it constitutes, on the contrary, a rejection which is effected only in regulated and detached portions. The new paper of M. Flourens is intended to shew that these two sorts of vomiting depend on different stomachs, and thence to arrive at the explanation of this extraordinary fact, that animals which regurgitate with most facility do not vomit unless with extreme difficulty, or even do not vomit at all. After having instanced the experiments of Daubenton, Gilbert, and Huzard, he details his own. We cannot dwell on them here. We shall confine ourselves to repeat the conclusions which he has drawn from them. From the facts and observations contained in his paper, the author concludes, 1°, that tartar emetic produces on sheep the same general effects, that is, the same excitation of all the powers which provoke or determine the vomiting which it produces in ordinary animals; 2°, that among the different stomachs of ruminating animals, it is on the rennet-bag, i. e. to say, on that alone, which by its functions as by its structure, corresponds with the simple stomach of other animals, that the emetic displays its action; 3°, that it is to the particular and altogether opposite disposition of this stomach in reference to those for regurgitation, that is to be ascribed on one hand the facility which ruminant animals have of regurgitating, that is to say, of throwing up into the mouth the substances contained in the first two stomachs, and, on the other hand, the difficulty which they have in vomiting, i. e. in rejecting and bringing back into the mouth the substances contained in the fourth stomach. If it be recollected that this fourth stomach is that where the definite conversion of the aliment into chyme takes place, that which contains the ruminated substances, the substances which consequently must no longer return to the mouth, whilst the first two stomachs, on the contrary, are those where the aliment undergoes only a certain preparation, those which contain only the substances not ruminated, the substances which, consequently, must return into the mouth, we shall soon see why every thing must be disposed to render easy the rejection of the two first stomachs, and that of the fourth very difficult. Without this arrangement, in fact, the ruminated substances contained in the

fourth stomach would be constantly mixed together, confounded and brought back into the mouth with the substances not ruminated, a confusion which must be an obstacle to the accomplishment of the end which nature proposed to herself to attain by the act of rumination.—*Archiv. General*, Feb. 1833.

Observations on the Medusa Marsupialis, by M. Milne Edwards.—There are few animals, the appearance of which excites more surprise than those gelatinous but animated masses which swim on the sea, and are designated by naturalists under the name of medusas. Their organization is no less extraordinary than their outward forms: the researches of M. Cuvier have shewn that some of these zoophytes have no mouths, but suckers similar to the roots of plants, and a digestive cavity extending in the form of canals through all parts of the body, and which discharges at the same time the functions of a heart and a stomach. Again, all zoologists agree in saying, that in other medusas there are neither mouths, nor suckers, nor stomachs, nor vessels, nor any distinct organs, and that the body consists merely of a species of homogeneous jelly.

Travellers have described a great number of these beings, apparently thus simple; but to the present period their structure had not been examined with all the attention which anomalies so remarkable merited; and the researches of M. Milne Edwards, intended to complete a part of this defect, tend to make one think, that often at least people have allowed themselves to be imposed on by appearances, and that the medusas called *agastrie* have in reality an organization much more complicated than is generally supposed.

The *Medusa Marsupialis* (Carybdée Marsupiale of Peron and Lesueur) is of the number of those zoophytes which it has been agreed on to consider a simple gelatinous mass, extended into membranes and curved into the form of a bell. Now, by studying it with care, M. M. Edwards has discovered in it not only a mouth furnished with tentacula, a stomach, and a great number of vessels, but, moreover, organs of a complicated structure, which he considers as being, the one, biliary canals similar to those which in insects serve for the liver, in others as ovaries.

This paper is accompanied with three plates, which shew; 1°, the aggregate as well as the particulars of those parts in the Carybdée Marsupialis, and in the Rhizostome d'Aldrovande; 2°, those of the different membranes which had been regarded as being the ovaries of the Rhizostome, and which, according to the physiological experiments made by the author, are organs of absorption and exhalation, as also the fringes which are attached to the claws, or tentacula, of these animals, and the partition which separates their stomach from the four lateral cavities of their body.—*Revue Medicale*, Feb. 1833.

New Observations on the Direction of Stems and Roots, by M. Dutrochet. The phenomenon of the ascent of roots, which the au-

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thor, in a preceding memoir, had noticed in the cactus, is particularly remarkable in the plants of the genus *pothos*. Of those considerably large roots which grow in the air at a short distance above the soil, some are often seen which have a vertical direction upwards, whilst others descend towards the ground. This circumstance was observed principally by M. Dutrochet in the *pothos*, *mexicana*, *crassi nervia*, and *digitalis*; he even saw in the latter species some roots which, after having been developed horizontally in the superficial part of the soil, became elevated all at once in the air, and pointed towards the sky. In all these roots, observation induced him to recognize a cortical system very considerable, and a central system very deficient. Now, this fact seems to contradict the principles laid down by the author in his preceding memoir, since, according to it, it is in virtue of the predominance of this cortical system that the roots assume a descending direction. However, this apparent contradiction, far from overturning the theory, is but a striking confirmation of it.

In fact, the general type of the organization of the cortical system is the decrease from without and within of the utricles which compose the system. It is this order of decrease which determines the incurvation inwards of this system, and consequently the descent of the stems in which this system predominates. Now, by a singular anomaly, the cortical system of the aerial and ascending roots of the *pothos* is found to be composed of utricles decreasing from within outwards in almost all its thickness: near the central system there is but an inconsiderable layer of utricles, which decrease in size outwards and inwards. The result of this arrangement is, that the cortical system, always predominating, as in the ordinary case, acts, however, in the opposite way, and thus its effect is to bend the roots up. The subterraneous roots of the *pothos* have the general organization of the roots, that is to say, their cortical system is composed of utricles decreasing from without inwards. It appears to M. Dutrochet that the contrary arrangement notable in the aerial ascending roots is owing to the drying power of the atmosphere, which tends to waste the vesicles with so much the more effect in proportion as they are nearer the surface. If this wasting does not penetrate too deeply, the roots descend, and the tendency they have to shun the light also favours the descending movement.—*Ibid.*

On the distinctive Characters of Siberian and Canadian Castor, by Guibourt.—All authors have distinguished two sorts of castor, that of Siberia and that of Canada; and several of them (as Geoffrey and Valmont de Bromare) merely say that the castor which comes to us from Russia and Poland, *viâ* Dantzick, is considered better than the other. Two very recent works give certain characters to distinguish these two sorts of products; but they are evidently mistaken in representing the Siberian castor as that principally used in medicine, and in describing as such that ordinarily found in commerce. For my part, up to last year, I had seen and described only the Canadian castor, which is the only species used in France and England;

and far from admitting with the above authors, that this castor is usually adulterated, and of a weak odour, I can say that it is almost always pure, and of a strong odour; I can add, that the variations in its plumpness, colour, and consistence, are most frequently owing to the age of the animal, or to the greater or less distance of the period at which it may have been killed from the time of rutting. Sometimes, in fact, the glandular apparatus, which forms the interior of the follicle in the castor is almost empty of unctuous matter, and sometimes it is entirely filled with it; so that these two castors, when dried, present an aspect altogether different, the first having a fracture entirely fibrous, whilst the second presents a smooth and resinous fracture, which allows the fibres and interposed membranes to be perceived only when the resinous matter is dissolved by alcohol. In general, the following are the characters of good Canadian castor: it is in follicles oblong, pyriform, corrugated, or flattened by desiccation, being from two to four inches in length; most frequently these follicles are joined two by two in the form of a double bag, but sometimes also they are single, or even four in number; oftentimes we find the penis dried and applied to one of the follicles. This castor is very hard, brittle, not friable, with a resinous fracture, red, or of a liver colour, having a strong and fetid odour, and an acrid, bitter, nauseous taste. Last year a French merchant, returning from Moscow, brought with him forty ounces of Siberian castor. Its origin is uncertain; and, besides, a Polish physician, to whom I shewed it, recognized it as the castor used in Poland and Gallicia, where it is very dear. The price of this was accordingly so high, that the druggist with whom it was deposited in Paris could not give it for less than eighty francs per ounce; and M. Blondeau and myself were, I believe, the only persons who procured half an ounce of it for forty francs. It could not, therefore, be sold, and the proprietor took his merchandize to try and sell it in England. Fortunately I had taken care to secure it, which, together with what I have remaining of it, will enable the Academy to become acquainted with Siberian castor. I do not pretend to say that this castor is pure, and ought to be preferred to that of Canada; far from it, I consider it to have undergone some preparation, which changes in all probability its natural state. All I wish to conclude is, that this substance is either that which is known in the east of Europe under the name of *Siberian castor*, or it is sufficiently scarce for its price to be ten or twelve times higher than that of Canadian castor. Here now are the characters by which it may be known: Instead of being in separate, oblong, pyriform, and corrugated follicles, this castor is in follicles which are full, rounded, more broad than long, and, as it were, formed of two follicles combined into a single one; only one specimen out of the forty ounces presented two ovoid follicles. Three-fourths separated, and the form of some others indicated an interior division, but nearly the entire presented a complete combination of the two follicles into one. This castor has an odour of an aromatic empyreuma, similar to that of Russia leather, very strong, and susceptible of great extension; it is only when

this odour is dissipated that the fingers which have touched it allow one to perceive that which is proper to the castor of Canada. It possesses a solid consistence, nearly dry and friable; it is yellowish, gritty under the teeth, has a taste not very perceptible at first, then very bitter, and with very little aroma. It forms with alcohol a tincture scarcely coloured, not only because it furnishes it with very little soluble matter, but also because it wants the red colouring principle of Canadian castor.

On the whole, these two castors differ in all their physical properties, sufficiently for one to suppose them possessed of marked differences in their therapeutic action; but it will probably be some time before we shall be able to compare them in this respect. As to their comparative chemical analysis, I have already commenced it, together with my friend M. Blandeau; but as yet it is not sufficiently advanced for me to be able to lay it before the Academy.--*Journ. de Chem. Med.*, Oct. 1832.

ANATOMY AND PHYSIOLOGY.

Double Vision, by Prevost.—“This imperfection of my sight,” says the author, “is dated from 1823; though it still exists, I shall speak of it from my notes, almost all anterior to the present period, because the state of my sight no longer allows me to observe with all the exactness desirable the phenomena it presents.” If M. Prevost views a point with the right eye, he instantly sees two; these two images are separated by an interval capable of measurement; in fact, in order to appreciate it, it is sufficient for him to compare with any object of known length, such as a printed letter (*caractere d'imprimerie*), a well defined line (*un trait bien limité*), &c.; at the distance at which he is accustomed to read, the distance of the two images appears to be half a line. But at such small distances it is hard to avoid some causes of error; in fact, according to the situation, one of the images becomes weakened, and may even become entirely effaced: the same eye which with the author is affected with a doubling of the objects, is also affected with a tripling of it. From these two circumstances it may follow, that at different distances the two images are no longer the same. It is then at great distances that we can hope to determine with any degree of exactness the separation of the two images observed. This has been done by M. Babbage, one of whose eyes is subject, as that of M. Prevost, to double a single point.

When one eye sees two images of any one single point, these images being on one and the same vertical right line, we know that the upper image ought to correspond to a point lower down on the retina. This the author has directly proved. By causing a screen to move

slowly from above downwards, he makes the lower image disappear before the other. It becomes gradually pale, whilst the upper acquires a deep tint equal to that of the effaced image. Thus the doubling disappears by masking one of the images. This image may be masked naturally by the eye-lid; the lower image by the upper eyelid, *et vice versa*. This species of natural screen is easily obtained by a mere inclination of the head. We have just said that the image masked by the slow and gradual movement of a screen becomes pale before it disappears. By successively diminishing the bundles of rays which come from the point to each of the two foci, and by graduating the movement, we cause the images to disappear by turns, and we meet with an intermediate inclination of the head or of the screen, at which the two apparent points are equally illuminated. The interval, which separates the two images from a black point traced on a white paper, assumes a yellow tint. In order to perceive it well, we must observe the doubling of a line. If we observe a coloured object of small extent, a small circle, or a small rectangle, it happens that we have the appearance of three tints by the effect of the double image. That takes place when the two images cannot be entirely separated. Where they remain confounded together, the tint is doubly strong. It is particularly in reading that the doubling of a small object is observed by M. Prevost; it may cause him to confound an 0 with the figure 8, but he rectifies this error with considerable facility, though this imperfection of sight causes him some embarrassment in reading. The doubling takes place on viewing the object with a convex glass, a line of three inches focus for example. "On bringing the glass near the object, such as a black point on a white paper, I see," says he, "two points, one over the other, nearly as with the naked eye; if I bring the glass near my eye, the interval of the two images diminishes, and they may even be confounded. At this limit I see but one image much more distinct; still a sort of semi-transparent shade surrounds it; but this shade is not perceived without difficulty. In continuing to bring the glass near to the eye, two images are again formed, and the more I approximate it, the more they separate: but they are not one above the other, they appear on a line almost horizontal; they are less distinct than the preceding, and soon become very confused."

M. Prevost then proceeds to inquire into the cause of this phenomenon. "Wherever this cause may be seated," continues he, "it produces two foci on the retina, to which the rays sent forth by one and the same point meet. This may happen in more ways than one. The simplest is that which places the defect in the crystalline lens. This organ ought to be, in this case, considered as a double lens. This effect may arise from a fracture, a bruise, a scalliness, or a partial flattening of the crystalline."

"The effect of a fracture is readily seen on a glass lens; it produces a double focus very apparent. It is to this cause that Wollaston referred a doubling of the image which he had occasion to observe; and in speaking of this affection of my right eye, he told me that he

had caused a momentary disappearance of the doubling by viewing the object through the refracting angle of a prism. Again, such a separation of the focus may take place without rupture; it would be sufficient that one of the segments of the lens be inclined on the other, so that the crystalline, for example, undergoes a wrinkle or fold (*un pli*) in its texture. May not this organ, composed as it is of layers, be exposed to accidents of this nature? If one of the segments were more flattened than the other, their foci would not be the same; but in this supposition the foci would not be exactly of the same distance, and would not fall on the retina so as to form there at once two perfect and distinct images.

“In recapitulation, there are some eyes made so as to see two images of one single point. These two images, measured on the visual angle, have been found distant from each other twelve minutes of a degree. The cause of this phenomenon ought to be a disjoining or separation of the crystalline lens, by a rupture, or a fold, or perhaps a partial change of convexity.”

To this extract from M. Prevost, we shall annex an analysis of a letter written to him by M. Babbage, who was attacked with a similar infirmity:—

“I see the two images of the objects vertically one over the other with the two eyes, or with each of them separately, every time I look at an object without forcing the organ. The upper image is weaker than the lower or real one, and is separated from it by an angle of twelve minutes. When I am in a bad state of health, the second image becomes stronger, but its angular distance is not altered as far as I have been able to judge. (The state of health does not appear to have any influence on the phenomenon in question with M. Prevost.) By reason of the small distance of the two images, the objects, when brought near, do not appear double, but an indistinctness is manifested on the edges. When I look through a small hole made in a card, or through a very small opening between my fingers and thumb, as I often do in order to see more distinctly, I lose sight of the weak image; I can also free myself of it by leaning the head back, and directing my view under the eye-lid, so that the ray coming from the object grazes it very closely. I see but one image on looking with one eye through a concave lens; I can also get rid of the weak image by frowning; but considerable exertion is required for that.”

This defect of vision does not appear up to this to have engaged the attention of any physiologist. Cabanis, however, speaks of it, and says that he observed two instances of it.* C. Bonnet, in his memoirs on his life and writings, a manuscript as yet unpublished, speaks, on the subject of his diseases of the eyes, of a doubling of the objects by that one of his eyes which he had devoted to the mi-

* *Rapports du Physique et du Moral de l'Homme*, 2nd edit., tom. 1, p. 525.

roscope. We read in the memoirs of E. A. Holyoke, a physician of Salem, in New Jersey, who died in 1829, at the age of 100 years, that in the latter years of his life the objects placed at a certain distance were multiplied to his eyes so that he thought he saw four or five moons. Finally, in a note added to the memoir of M. Prevost, M. Arago informs us, that his left eye presents to us all the phenomena described by the professor of Geneva, but with some circumstances which appear to him to modify the explanation given of them. He promises to return to this subject at another time.—*Arch. Gen.*, Jan. 1833.

Hermaphroditism.—M. Isidore Geoffroy Saint-Hilaire presents a manuscript memoir entitled, “Anatomical and Physiological Researches on Anormal Hermaphroditism in Man and Animals.” The author considers the generative apparatus altogether as formed of six principal segments, which in several circumstances are independent of each other, to wit, of the right and left side; these are, 1st and 2nd, *deep-seated* organs (ovaries, or testicles and their appendages); 3rd and 4th, the intermediate organs (the womb or prostate, and the vesiculæ seminales and their appendages); 5th and 6th, *external* organs (clitoris and vulva, or penis and scrotum). The facts which the author states establish the independence of these six segments, and shew, that there is not one of them which may not present sexual characters the reverse of those of all the rest. These six segments correspond to six orders of different vessels; the deep-seated to the spermatic arteries, the intermediate to the branches of the hypogastrius, and the deep-seated (*les profonds**) to branches of the external iliacs. M. Geoffroy divides the numerous cases of hermaphroditism into two grand classes, hermaphroditism without excess, and hermaphroditism with excess. He subdivides the first class into four groups, to wit, 1st, *masculine hermaphroditism*, the generative apparatus being essentially male; 2nd, *feminine hermaphroditism*, the generative apparatus essentially female; *neuter hermaphroditism*, apparatus without determinate sex; 4th, *mixed hermaphroditism*, apparatus presenting a real mixture of the two sexes. He admits these subdivisions in the second class: 1st, *masculine hermaphroditism complicated*; 2nd, *feminine hermaphroditism complicated*; 3rd, *bisexual hermaphroditism*. M. Geoffroy then passes these several genera in review. From the facts and observations contained in his papers the author draws the following conclusions.

Perfect hermaphroditism, in the anatomical sense of the term, has never been observed. The most complex cases are those, where there exist double organs deep-seated and intermediate, the one male, the other female, and in fact the penis and the clitoris, by reason of their connexions with the several bones of the pelvis, could not co-exist without a serious disturbance of all the connexions.

* *Quære externes?* the external.—Ed.

As to perfect hermaphroditism, in the physiological sense of the word, its possibility is incontestible in animals, as in fishes, which have the two halves of the sexual apparatus quite separated from each other in the normal state, and in which there is no copulation.

The frequency of hermaphroditism in general, and of each kind of hermaphroditism in particular, is very different according to the groups of animals: thus in man, masculine and feminine hermaphroditisms, particularly the first, are very rare.

“With reference to legal medicine, it is sufficient for me to point out here,” continues the author, “the insufficiency of the precepts given by authors for the determination of the sex in doubtful cases, precepts which have appeared exact only because there had been but a very few of the combinations distinguished which nature presents. This difficulty in distinguishing the sex is the consequence of this general fact, that whilst the internal organs vary almost to infinity in number, structure, and arrangement, the external ones preserve their normal number, and the modifications which they present in other respects being intermediate between the male and female sexes, are included within limits sufficiently narrow. It is then impossible, that a particular arrangement of the external organs could correspond to each of the special combinations of the internal organs.”

Lastly the author remarks, that legislation admitting only two grand classes of individuals on whom it imposes duties, and grants different and almost opposite rights according to their sex, does not truly embrace the entire of the cases; for there are subjects who have really no sex; such are neuter hermaphrodites, and hermaphrodites mixed by superposition; and on the other side, certain individuals, the bisexual hermaphrodites, who present the two sexes united in the same degree.—*Archiv. Gen.*, Feb. 1833.

Fissures observed in the Bones of the Cranium of new born Infants, after natural Labour, by J. De Siebold, Professor of Midwifery in the University of Marburg.—Medical jurists (*medicins ligistes*) were formerly in the habit of instantly pronouncing, that external violence had been used on the head of the fœtus, when the latter exhibited traces of openings, fissures, &c.: Haller, Bosc, Berchols, and others, positively asserted this opinion. Deventer, Dionis, Roderer and Baudeloigne, are the first accoucheurs who remarked that these affections may coincide with a labour altogether natural. Schmitt, in a periodical publication; Hist, in a dissertation entitled, *De Cranii Neonatorum Fissuris*, &c. commentatio Leipsic, 1815; Osian den, d'Outrepont, Meissner and Carus, in their writings on labours, establish the truth of this by numerous examples. Medical jurists reconsidered their assertion, and the possibility of the fact is now beyond doubt. Nevertheless, as the cranium of new-born fœtuses is not minutely examined, except in cases of medico-legal investigation, it so happens that science as yet possessed but few well detailed observations on this subject. The following leaves nothing to desire in this respect.

A woman 30 years old was admitted 11th March, 1831, into the lying-in-house of Marburg: she was pregnant of her third child. The first had been extracted, already dead, with the forceps. The second, dead as the first, had been expelled under the influence of the ergot of rye, administered in a dose of 30 grs.: there had been impaction of the head; deep depression of the temples, and ecchymosis on all this part, were sufficient proofs of it: this labour took place in February, 1829; the body of the child was not examined. On admission, the woman was in the following state: the uterus was three inches beyond the umbilicus, the neck very high and obliterated; Bandelocque's measure gave a diameter of three inches and a half, and we could not reach the sacro-vertebral symphysis with the finger. The pains commenced the 5th of May, in the morning; the waters broke towards 5 o'clock; the orifice of the uterus diminished almost instantly, as generally happens when its neck is merely dilated by the membranes, and is not replaced at the moment by the head of the child. The same took place in this case, and we could satisfy ourselves of the position of the parts. The head presented at the brim, according to its transverse diameter, the right parietal bone was before, the lesser fontanelle on the left, the great fontanelle to the right, the head firmly supported on this side. The pains returned with renewed force, but the position did not change, the promontory was passed with difficulty; at last the head entered into the lesser pelvis, was placed in its first position, and at half past nine at night the fœtus was expelled. It weighed seven pounds, and gave no signs of life. On the following day the body was examined. The longitudinal diameter of the head was four inches and a half, the transverse three and a half, the diagonal 5; an enormous bloody tumour occupied the right parietal, and the left temple was denuded of its epidermis. The parietal of the same side presented three fissures, one an inch and a half, extending from the parietal protuberance towards the anterior extremity of the sagittal suture: the two others much shorter, extended from the tube towards the parieto-frontal suture. A fourth fissure, an inch in length, existed in the left half of the coronal suture towards its lower part. Again the parietal crossed below its fellow, and below the corresponding coronal suture; a considerable quantity of blood passed through the fontanelles; the brain was not examined.

No one will doubt that the pressure on the sacro-vertebral symphysis was not the sole cause of the observed fissures; it is a new proof of the numerous means employed by nature to terminate labour. If the forceps had been employed, it is certain that these appearances observed would have been attributed to it, and it must be allowed that in a case of forensic medicine, it would have been difficult to decide whether there had been any external violence: attentive examination of the lesions, that of the woman's pelvis, could alone furnish any probability. In conclusion the author puts the question, what should be

done if the woman again became pregnant? He declares against forced delivery, which, he says, is always attended with fatal consequences to the mother: he conceives that the most rational treatment would be to prevent by small bleeding, gentle laxatives, and moderate diet, the too great development of the fœtus.—*Journal d'Accouchement de Siebold*, 11th Vol. 1831.

PATHOLOGY AND THERAPEUTICS.

Intermittent Syncope, by Dr. Richelot.—Félicité Condere, inhabiting from her birth a marshy country six leagues from Bourges, in Berry, had always enjoyed good health till the age of 23 years: appetite good, digestive functions well performed, the menses had never undergone any alteration, and the muscular strength was sufficiently developed to render her capable of wrestling with men of her own age. She was of the ordinary size and in moderate condition of body; when twenty-three and a half years old, on making an effort to raise a bed, she felt an acute pain in her left side: rest did not cause it to disappear: respiration became difficult, and at the end of two days a physician was consulted, who ordered nine leeches to the painful part. This pain never completely ceased: from time to time it became more distressing, and aggravated particularly by violent exertion. She lived in this way for six months without her general health being altered in a manner so as to excite alarm: at the end of this time she began to experience considerable uneasiness, pulsation in the epigastrium, a dry and distressing cough, soon followed by the nervous phenomena I am about to describe.

The day her illness commenced, she had not been able to breakfast for a reason wholly unconnected with her health, and this forced abstinence caused her to suffer much; she dined without appetite at 6 o'clock in the evening, and three hours after she was all on a sudden seized with a nervous attack (*attaque de nerfs*) which lasted without interruption till the following evening, for nearly twenty-four hours. Several persons united their strength to hold her. A cough which seemed likely to suffocate her was also attended with violent contractions of all the muscles. This *nervous attack* differed not from other neuroses except in the intensity of the morbid phenomena, and in its length; it ceased at the end of twenty-four hours: the night was passed favourably, but from the day after the nervous attacks became regular: a similar attack commenced at 8 in the morning, and lasted till noon: a second seized her at 8 in the evening, to cease at midnight, and from this period two attacks took place regularly every day at the hours and in the manner pointed out, and similar in every respect. Antispasmodics and antihysterics were administered in all forms; lavements, baths, leeches to the thighs and

legs, blisters to the arms; a strict regimen was prescribed; the sulphate of quina was also administered. So little success was experienced from all those remedies, that at the end of some months the attacks became more frequent, and increased in violence. A pain was felt in the left knee, and the corresponding leg was seized with a continual trembling: some time after the head became the seat of irregular and involuntary movements. At the end of a year the patient would no longer take any medicine, and resigned herself to her wretched fate. In the interval of the attacks her health was not bad, she slept at night and had a tolerably good appetite. It is worthy of remark, that her menstruation was never deranged. There soon came on a curvature of the spine. However, an important change was gradually effected in the disease. The attacks became more and more frequent, but they lost their violence, and degenerated into true syncopes. The spine became straight, the irregular movements of the head and left leg disappeared, the patient became able to walk without limping; in a word, her health became less distressing. This change took place about 18 months after the first attack, and I had an opportunity of seeing the patient about three years after. I several times witnessed these syncopes. They then took place eight times in the 24 hours; at 4 o'clock, at 6 o'clock, at 9 o'clock, and at 11 o'clock in the morning, then at 4, 6, 9, and 11 o'clock at night. In the interval from 11 o'clock at night to 4 in the morning, her sleep was usually good. The syncopes took place thus: she feels a general uneasiness and a sense of constriction in the epigastrium. She yawns, becomes pale, her voice fails her, then her strength gradually declines to a complete loss of all voluntary motion; her eyes close, all the faculties are suspended, and the pulse becomes a little slow. If she be left in this state, she remains so till the following syncope: in order to rouse her she is struck on one of the hands, and at the end of some minutes she begins to make slight motions; then she throws her limbs, utters some cries; wind issues in great abundance from her chest from 5 to 20 minutes, and at last she opens her eyes. From the first yawnings to the moment when she recovers her senses, an interval elapses of from 15 to 20 minutes.

Such is the state in which she lived for three years, when I saw her for the first time. In this time she had a chest affection which lasted for 15 days, but which produced no alteration in the ordinary nervous phenomena. To the latter was added from time to time the convulsive cough which she experienced at the commencement of her sickness, and the sensation of a lump which seemed to her to ascend to the throat. The appetite was rarely good, digestion was very difficult, and the stomach very painful. However, solid food passed in easily enough whilst liquids were very often rejected. As she had taken the resolution of attending to herself no longer, she had several attacks of indigestion, which obliged her to diminish and select her food with more care. By means of this precaution she suffers little in the interval of her syncopes. The latter are much more severe, and her general health is much worse when she has any uneasiness

of mind, or even when she is thwarted in any way. For the first time of her life she was attacked with a tertian fever, which followed its course independently of the other morbid phenomena. Notwithstanding the state of the stomach, I prescribed the sulphate of quina at once to cut short the fever, and to see what influence this medicine might have on the general health. The result appeared satisfactory: the fever ceased completely, and from that period the syncopes became less in duration, and that which occurred at 11 o'clock in the morning ceased to take place, so that instead of eight in the 24 hours, she has but seven. I know not how far we can attribute this slight amelioration to the weak dose of sulphate of quina which the patient took, but I could at least try this treatment without apprehending from it any mischievous results. The surest means of obtaining success from the employment of any medicine, and particularly from sulphate of quina, is about to select the most favorable moment for its administration. In the curious case before us, one can accurately recognize an intermitting disease, presenting the double quotidian type. The first access lasts from 4 to 11 o'clock in the morning, the second from 4 to 11 at night; these invasions correspond to the *nervous attacks*, of which they took the place, and are themselves composed of a fixed number of partial invasions, which have a tendency to be confounded into one, as we might have seen above, since it was necessary to strike the patient in the hand, that the syncope may not be prolonged to the following. Thus the period of remission should be taken between 11 o'clock in the morning and 4 in the evening, between 11 o'clock in the evening and 4 in the morning. According to these considerations, some months after my first visit, I prescribed the sulphate of quina in a dose of 8 grains twice a day, the first dose to be taken immediately after the last invasion in the morning, and the second after the invasion of 11 o'clock at night. The disease had then existed 5 years, dating from the first nervous attack. A very strict regimen, &c., were prescribed; I advised her particularly to contrive that she might never know the hour, and to give up the custom she had contracted for a long time of preparing herself at the time when she awaited the fatal hour to strike. These precautions and the medicine had no result either favorable or otherwise; and the patient, readily inclined to be discouraged, refused at the end of some days to continue to attend to herself.—*Archiv. Gen.*, Jan. 1833.

Hydrocephalus with Tubercle and Polypus in the Cerebellum in a Child 4 Years' old, by Dr. Guillet.—Cardinal, James Philip, presenting the appearance of a very meagre child, with emaciated limbs, was born after seven months' gestation. From the time of his birth, I remarked on the fontanelles a well-marked prominence, as in cases of hernia of the brain. During his life-time, this child was delicate and sickly, and a prey to a slow, continued fever. From three to four months bleedings from the nose; a progressive predominance of the head over all the other parts of the body; dentition slow; not till he was thirteen months old did the first incisor teeth appear.

This period of life could not pass without being attended with disturbances of every kind : diarrhœa, nervous trembling of all the limbs, general convulsions, enlargement of the cervical glands, &c. At a later period, towards the second year, in proportion as relative life became more developed, it was remarked that the child always sought some point of support to rest his head, and when he was deprived of it, he supported it with one of his hands. In order to look to the right or to the left, he could not make his head perform the motion of rotation on its axis: thus was he obliged to move the entire trunk, as persons do who are affected with wry-neck. After a lapse of two years, his head was larger than that of an adult: its frontal region was very much flattened; as if by way of compensation it presented laterally an enormous swelling which occupied the parietal, temporal, and occipital regions principally.

At the age of three years the child got a fall, of which the head received the entire shock. From that time he uttered the most plaintive cries; felt acute and continued pain, which he referred to this part of the body. Total dislike of bodily exercise; averse from mixing in the amusements of children of his own age, he was moody and fretful, and remained constantly sitting or lying. However, if the life of relation began to lose its activity, such was not the case with organic life, he ate and drank heartily. Since the time of the fall, the convulsions were renewed with much more frequency; one day, during one of these convulsions, he vomited some lumbrici. Four months after the fall, the child was suddenly struck with hemiplegia of the left side, the side to which the head usually inclined. The arm and leg of this same side were deprived of motion; the tongue participated in this paralysis. In speaking, each word was interrupted by a moment of silence, as if the brain needed an instant of rest for the elaboration of its ideas. A few days after the appearance of this hemiplegia, the little patient was struck blind, the pupils retracted, fixed, and the eyes a little dull.

At three years and a half the paralysis became complete in the lateral parts of the body; the right hand was the last part of the limbs which was attacked. From this new occurrence a very strong contraction of the fingers resulted. The bladder was also paralysed: thus, there was an incessant incontinence of urine. Reduced to this deplorable state, the child sometimes remained 48 hours without desiring to take food, nor even drink. On emerging from this extreme indifference for every thing connected with the wants of life, he still evinced by some words badly articulated, by some cries, or by signs, a desire to take food.

Five weeks before death, the faculty of hearing, which up to this period still existed, became totally extinct. Thus no power of motion, no vision, no speech, no signs, no hearing: there remained to this unfortunate being nothing but a faint cry. Sugar and water were his only nourishment. After numerous fits which were renewed every day, complete loss of motion and tetanic rigidity ensued. Occasionally the child preserved the consciousness of *me*, for he was able

still to form a judgment, confused no doubt, with respect to the position in which he was placed. As soon as his mother laid him on his bed, he immediately gave utterance to some plaintive sounds; after taking him in her arms, the cries immediately ceased. He died on the 12th of September, 1832, in the midst of a violent fit. He had attained his 4th year. The therapeutic means employed by me were as follow: pediluvia, leeches beneath the mastoid processes, sinapisms to the feet; a blister to the nape of the neck, and also to the head; infusion of lime (*tilleul*), barley water; vermifuges; demulcent and antispasmodic drinks; mild injections, &c.

Inspectio Cadaveris.—The body was opened 24 hours after death; cadaveric rigidity well marked; upper and lower extremities strongly contracted; fingers of the left hand so bent on the palm, that the skin of the part was excoriated by the nails; general emaciation even to marasmus: thus the child lived for a considerable time at the expense of his own substance; eyes dull, and deeply sunk in their orbit; pupils retracted; crystalline opaque; cheek-bones very prominent; in a word, the face completely hippocratic.

Size of the head enormous; its form that of a sphere flattened at the forehead, with little development in the fore part, on the sides very projecting, and also behind. On touching it, it was ascertained that the fontanelles produced by a want of approximation of the bones of the cranium, extended an inch and a half in their transverse diameter. On opening the skull, I found the dura mater transformed into a fibro-cartilaginous tissue, the resistance of which must have prevented the formation of cerebral hernia. When the scalpel penetrated this membrane, about two ounces of a clear serous fluid flowed out. The vessels delineated on the cerebral hemispheres, as well those which were ramified in its substance, were gorged with blood. On making slight pressure with the fingers on the two lobes of the brain, we perceived the fluctuation of a liquid effused into the lateral ventricles. After opening these two cavities, I made the following remarks: the cerebral substance possessed its natural consistence; the ventricles, whose parietes were of a dull white colour, were enormously enlarged; these cavities contained a serous liquid, very clear, the quantity of which may be stated, without exaggeration, at 14 ounces; and, finally, the optic thalami, the corpora stricta, the cornua Ammonis, and the choroid plexuses, were developed in the direct ratio of the extent of these cavities.

In the right lobe of the cerebellum, I found a tubercle the size of a hen's egg, of a rounded form, and a yellowish colour. Its tissue was on the outside in a state of crudity and softened in the centre. This morbid production, developed at the expense of the substance of the cerebellum, (for it replaced all its anterior part which was absolutely wanting), was in immediate contact with the membranes. On separating the brain from the spinal marrow, I found resting on the basilar surface of the sphenoid, a polypous vesicle the size of a nut, adhering to the cerebellum, which I was able to crush between my fingers. The left portion of the cerebellum presented nothing worth

noting. This case, which certainly is not novel in its kind, fails not, I think, to present some interest, with respect to the coincidence of the different alterations which it presents, and with respect to that of the progress of the phenomena depending on them. Serous effusion, tubercle and polypus of the brain, such is the nature of the affection; disturbance of innervation, convulsions, progressive paralysis, and death, such the consequences of them. When we consider, that in a crowd of cases of cerebral apoplexy, even where pathological anatomy discovers nothing to our senses, the patients are suddenly struck, has not the physician reason to be astounded at seeing persons drag out an existence for whole years, in whom there is a series of disorders as marked as in the case before us? How, in fact, can we conceive, if the symptoms were also in proportion to the extent of the lesions, that this unfortunate child could so long sustain a vegetable life, in whom most of the sensorial organs were successively struck, as it were, with a separate death? Whilst waiting for satisfactory explanations on this subject, let us agree that there prevails much over certain diseases, with regard to the relations of the symptomatic effects with their morbid causes.—*Archiv. Gen. de Med.*, Feb. 1833.

New Mode of treating Phthisis, by Dr. Giovanni de Vittis, Head Physician of the Military Hospitals.—This case is extracted from the work which this physician is after publishing, to make known the remarkable and unexpected results which he has obtained by a treatment which he has adopted in all the cases of phthisis which presented themselves at the Military Hospital of Capua, where most of phthisical cases of the army are received. The number of patients with whom Dr. Giovanni has adopted this treatment is as follows:—

“From the 1st May, 1828, to the 18th January, 1832, there went out of the Military Hospital of Capua, entirely cured, forty patients affected with chronic catarrh, forty-seven attacked with phthisis in the first stage, 102 who had reached the second stage, and twenty-seven whose disease had reached the third. Total—216.” The treatment consists in the administration, morning and evening, of a spoonful of the following solution: \mathcal{R} tartar emetic, gr. iii.; infus. of elder flowers, unc. v.; simple syrup, unc. i.

The patients drink milk and water in the course of the day. The regimen pursued by the patients consists of rice boiled to the consistence of pap, (*bouillie*), and sugar. With respect to those in the third stage with colliquative diarrhœa, they are given, instead of this, cream of rice, two cups of chocolate with two biscuits, one in the morning and the other at night.

It must be remarked, that, when the solution of tartar emetic has not excited vomiting at the end of a quarter of an hour, a second spoonful is sometimes given. It is particularly with individuals of a robust constitution that it is necessary to repeat the dose of the medicine. Care must also be taken to suspend its administration for some days with patients in whom it has produced repeated alvine evacua-

tions; during this interval they are made to take a mixture of ipecacuanha roasted, and digitalis purpurea, which immediately suspends the diarrhœa. The form of its administration is this: \mathcal{R} . *Digital. purpur. pulver. gr. x.*; *ipecac. torrif. et pulveriz. gr. x.*; *misce, et fiant pilulæ decem.* The patient to take one every hour, or at nearer intervals, according to the number of evacuations, and until they have ceased.—*Archiv. Gen.*, February, 1833. (*From the Annali. Univ. di Med.*, December, 1832).

Absorption of the Placenta. A Case furnished by Professor Nægele.—A woman who had already been delivered of three children at the proper time and with ease, terminated her fourth pregnancy by a premature confinement, brought on by mental distress. The child was dead, miserably small, and sixteen inches in length. The after-birth remained in the uterus, where it adhered to the lower part of the right side. There were neither hemorrhage nor pains. The ergot of rye was given in doses of fifteen grains every hour, without any apparent success. The uterus, strongly contracted, was about the size of a fist; the woman was doing well; the lochia were red, without any bad odour; the orifice of the uterus was so narrow, that one could scarcely introduce the fore finger into it. During the examination, the umbilical cord, and a shred of the membranes, five inches long, detached themselves, and were extracted. The use of the ergot was continued, but it was soon abandoned, in consequence of the disgust it excited; there were no pains, and nothing passed out by the vagina. In the night of the fourth day, a slight milk fever manifested itself; the lochia continued, not very copious, pale, and not foetid; the orifice of the uterus completely closed, and the woman was getting better and better. Seven weeks after, the menses re-appeared, and returned three times; then the woman became pregnant again, and was delivered happily, and at the regular time, of a strong live child. The after-birth soon followed.—*Archiv. Gen.* (*From Heidelberger Klinische Annalen*, t. vii. 3^e cah., 1831).

Cause of the Variation of the Obstetrical Action of the Ergot of Rye, by MM. Boettcher and Kluge.—The obstetrical properties of the ergot are even still so much controverted, that all the observations which can fix the opinion of practitioners on the degree of its efficacy ought to be collected. We here present some new facts which will tend to illustrate this point of therapeutics.

M. Boettcher, apothecary at Menselwitz, in the Duchy of Allenburgh, having thought that the diversity of the action of this medicine might depend on the period at which it was collected, got in a certain quantity of it *before* and *after* the harvest, so that in the first case he took away the grains of the ear while still in the ground, whilst in the second he gathered them in the threshing-floor, where the rye was threshed. He directed the separate products of these

two crops to the Minister of Public Instruction at Berlin, who remitted them to Doctor Kluge, Head Physician to the Hospice de la Maternité. The medicine was administered to 15 women only, the quantity not being large enough to allow it to be given to more. That the ergot might not produce any bad effect in the mother or child, Dr. Kluge took care not to administer it until the neck of the uterus was beginning to dilate, that the pelvis should be well formed, and that the child should be placed in a favourable condition, the only circumstances under which we can obtain favourable results from the use of this medicine. The results of his comparative experiments:—

1st. The action of the ergot collected *before* the harvest was very energetic, whilst there was no activity in that collected *after* the harvest. 2nd. In several cases the use of the first renders unnecessary the employment of the forceps, particularly when the insufficiency of strength results from real atony, or a spasmodic contraction of the neck. 3rd. The ergot of rye collected before the harvest possesses the property of preventing uterine hemorrhage, and if the application of the forceps was necessary in certain cases, where the pains had entirely ceased, this medicine may be opposed with advantage to the loss of blood which sometimes comes on at this time in abundance. 4th. The dose is from thirty to sixty grains, administered in ten grains at a time every ten minutes.

The experiments of Dr. Kluge have then confirmed the conjectures of M. Boettcher, and may explain the diversity of opinions which exists with respect to the obstetrical action of the ergot of rye. The manner of preserving it also exercises considerable influence on its efficacy. We know that Dr. Ryan, of London, has ascertained that when this medicine is exposed to the air, it loses all its qualities, whilst it may continue very active two years after its collection, if care be taken to keep it in bottles hermetically sealed.—*Archiv. Gen.* (From the *Allgemeine Medicinische Zeitung*, 10th November, 1832).

Chemical Analysis of the Ergot of Rye, by A. A. L. Wiggers.—

The experiments made by the author have shewn him that the ergot of rye contains in great abundance a thick oil, a vegeto-animal substance so susceptible of decomposition, that it evolves ammonia by its mere distillation with water; and, lastly, a particular colouring matter. In 100 parts of the ergot of rye Mr. Wiggers has found—

A white thick oil	35,0006
Fatty matter, rather soft, crystallizable, white, of a particular nature	1,0456
Cerine	1,7578
Spongy substance	46,0862
Ergotine	1,2466

Vegetable osmazome	7,7645
Sugar of the ergot of rye	1,5530
Extractive gummy matter, azotized, combined with a red colouring principle	} 2,3250
Vegetable albumen	
Acid phosphate of potash	4,4221
Phosphate of lime, with some traces of iron	0,2922
Silica	0,1394
	<hr/>
	102,1930

Vegetable osmazome being found only in mushrooms, from its presence here we may conclude that the ergot of rye belongs to this class of vegetables. Of all the substances contained in the ergot, water dissolves only the osmazome, the phosphate of lime, and the gummy extractive matter. The special property of this substance must then reside in the osmazome; accordingly, we have often observed that the decoction of the ergot was very energetic. The experiments of M. Wiggers prove that the ill effects of the ergot are owing to the ergotine which is insoluble in water. Osmazome, after being given to several animals, has not produced any effect.—*Ibid.* (From the same Journal, 14th November, 1832).

Papular intestinal Eruptions, by M. Burnet.—M. Burnet reports five cases met with at the Hopital des Infans, in 1829, of an internal papular eruption in every way similar to that described by the physicians of the north in cholera, and in which M. Serres has placed the anatomical cause of this epidemic, which he has for this reason called *psorenterite*. M. Bouillaud assures us that he has himself often met this eruption during the last twelve years. M. Duges, in his essay on the Nature of Fever and Inflammation, has published similar facts also collected at the Hopital des Enfants. These papulæ present the following anatomical characters. They occupy the tissue of the mucous membrane, are observed indistinctly on all the circumference of the intestine, and are innumerable, are placed more on the free edge of the valvulæ conniventes than between them; have been seen in the stomach and œsophagus; they are full, and without a central orifice, without any vascular circle surrounding them; are attached to the mucous membrane by a sort of short, flattened pedicle, which may be better seen by immersing in water the portion of intestine containing them; their volume is nearly that of a grain of millet; their tissue white and firm. These facts, and the consideration that the cholera patients, in whom they were not found, are more numerous than those in whom they were seen, upset M. Serres' theory. When will people renounce the folly of localizing every thing, and of wishing to place in a nervous ganglion, in some vascular injections, or some other alteration still less important, which is often but an effect, a mere accidental phenomenon, the special seat,

the principal commencement of one of those diseases which the ancients though not as good anatomists, yet better logicians than we, had so properly called general diseases.

Ferro-prussiate of Quinine in Ague.—At the close of the year 1831, Doctor Brutti, of Cremona, wishing to obviate the disadvantages attending the use of sulphate of quina in agues, particularly when they are accompanied with irritation of the abdominal viscera, tried to substitute for it the hydrocyanate of Quina: but though the asthenic properties of this latter salt should have made him prefer it to the sulphate, its facility to be decomposed, rendering the results of its administration less exact, caused it to be rejected by Doctor Cerioli, in the trials which he has since made; and from these experiments it follows, that the ferro-prussiate of quina being decomposed with much less facility, ought to obtain the preference. The following is an abstract of the work published by this physician: though in affections purely inflammatory the sulphate of quina has been used by several physicians, it is, however, pretty generally known that its principal property consists in its power of destroying periodical returns; and as agues are often accompanied with an inflammatory state of the digestive organs, the sulphate of quina, essentially sthenic, is but ill opposed, for that very reason, to the equally sthenic nature of the fever, or of the organic cause to which it is owing. Thus we see this medicine fail in these sorts of fever, against which, on the contrary, the ferro-prussiate of quina, which possesses at the same time the anti-periodical properties of quina, and the asthenic properties of hydrocyanic acid, succeeds very well. This Doctor Cerioli establishes by very conclusive cases in which the ferro-prussiate of quina removed agues against which the sulphate had been ineffectually employed for a shorter or longer time. This superiority of the ferro-prussiate over the sulphate has been principally observed in subjects labouring under fevers kept up by irritation of the abdominal viscera, and Doctor Cerioli has proved it in twenty-four patients, whose fever had not been at all cut short, or after having been so, again returned. In thirteen of these which were affected with quartan ague with painful engorgement of the viscera, the disease continued in some several months, in others two or three years, and even eight years in one young girl, without yielding to the most rational treatment, bleedings, local and general, demulcents, and lastly to the sulphate of quina, the use of which had either produced but very slight amendment, or none at all. In the cure of these fevers with the ferro-prussiate, M. C. administered it in the dose of 2, 3, 4, and even 8 grains a day: he divided this quantity into six pills, into which the rob of elder entered: he increased from two grains when the stomach easily bore the action of this medicine, or when the fever diminished merely in violence. Seldom after the use of this medicine did he witness any relapses, or what is very unusual with the sulphate of quina, any exacerbation in the abdominal irritations accompanying the fever; which M. C. explains by

the depressing action of the ferro-prussiate. In fever patients affected simultaneously with the ague and with chronic inflammation of the liver, he obtained from the ferro-prussiate a cure which he had not been able to effect by frequent bleedings, leeches applied to the right hypochondrium, epigastrium, and arms, followed by other antiphlogistic measures. In a word, it follows from M. Cerioli's work, that the ferro-prussiate of quina, besides the advantage it possesses over the hydrocyanate, of being less readily altered, and consequently of giving rise to more certain results, has this immense advantage over the sulphate, of being applicable to agues during an inflammatory state of the viscera, or accompanied with irritation or congestion of the *primæ viæ*.

Mr. Bertazzi, an eminent chemist of Cremona, has given the following process for obtaining the ferro-prussiate of quina: take one part of sulphate of quina, which is to be triturated in a glass mortar, so as to reduce it to a very fine powder: then add this to one part and half of ferro-prussiate of potash dissolved in six or seven parts of boiled water: after having mixed these substances anew, expose to the fire in a phial this compound, shaking it from time to time, till the liquid boils. The solution throws down to the bottom of the vessel a substance of a greenish yellow, having an oily consistence. After decanting the liquid, this substance is to be washed in distilled water, for the purpose of separating it from the undecomposed sulphate of quina, as well as from the hydrocyanate and sulphate of potassa, which might also be combined with it: the washing is concluded, and the water been thrown away, this substance is dissolved in very fine alcohol, which is to be exposed to a temperature of 30° Reaumur, to effect its solution: then filter, and by evaporation there is obtained a substance confusedly crystallized in needles, the weight of which corresponds to the three-fourths of the sulphate of quina employed. This substance when dried is of a greenish yellow colour, and of a very bitter taste, first giving the sensation of quina and then of hydrocyanic acid. The decomposition of the ferro-prussiate of quina is partial in cold water, and nearly complete in hot water: it then forms two salts, the one soluble, and the other insoluble. The alcohol dissolves it in the cold, but dissolves it still better when boiling. Such are the physical, chemical, and therapeutical properties of ferro-prussiate of quina which may be regarded as a valuable acquisition for the treatment of agues complicated with an engorgement, or chronic inflammation of the abdominal viscera.—*Revue Med.*, Jan. 1833, from the (*Annali Universali de Medicina*, tom. lxiii. p. 5.)

Extraordinary Effect of a Fall in the Cure of Phthisis, by Dr. De Simoni.—The inhabitants of the Canaries who come to reside at Mozambique, are extremely meagre. Amongst them was a soldier affected with a cough of long standing, sometimes dry, at other times accompanied with purulent sputa tinged with blood. This patient had a slow fever with exacerbations at noon and at night; his skin was dry, moistened only by the night sweats, and a burning heat peculiar to certain tuberculous individuals appeared to indicate

the existence of pulmonary phthisis. The voice nearly extinct and hoarse, pectoriloquy and a diarrhoea, which stopped at intervals, confirmed the fatal diagnosis, and declared sufficiently the incurable nature of the disease. It happened, however, that being put under a treatment purely palliative, he gained some little strength, and asked to leave the hospital in spite of the representations of the physician. It was not without considerable difficulty that he regained his abode and resumed his service. One day being placed as sentinel on an elevated part of the fortress, his strength failed him all of a sudden, and he fell with his gun in a passage covered with stones; no fracture resulted from this fall, but merely severe bruises in different parts of the chest. Soon after hemoptysis came on, then epistaxis so considerable as to cause alarm for his life. Baths were prescribed, with the addition of camphorated spirit: an infusion of centaury for tisan, and the application of some leeches, with nutritive diet. Under this treatment all the symptoms of phthisis disappeared gradually: the patient recovered flesh, and was perfectly restored to health.—*Rev. Med.*, Jan., 1823.

Practical Reflections on Purpura, by M. Genert.—The author thinks that purpura incorrectly ranked by Willan amongst the cutaneous exanthemata, presents itself under very different forms, which, consequently, indicate different modes of treatment. These circumstances, according to M. Genert, may be referred to three classes. Some cases, he says, exceedingly simple, are not accompanied by any constitutional symptom, cannot be attributed to any appreciable cause, and yield under the influence of the most opposite means, without our being able to know whether those which have been employed have been serviceable or not, or whether they have had any effect. In other cases the purpura is connected with affections most frequently severe in themselves, and adds to the unfavorable nature of the prognosis. These cases are especially those where adynamia prevails, typhus and typhoid fever: we have seen it complicate several other diseases, and among them a severe case of measles, of which it rendered the diagnosis difficult. In fine, there is a third order of cases, where, at the period when the purpura spots appear, we do not observe any serious symptoms, oftentimes not even any general disturbance; but presently, when proper treatment has not been adopted, sometimes in despite of the best treatment, there come on symptoms more or less serious, which in some cases end in death. The author thinks he may conclude, from all the facts which he states, and in general, that, 1st, In the state which approaches more or less near to scurvy, but is sometimes concealed under an appearance of health, and of which the purpura spots are frequently the first intimation, blood-letting is in general contra-indicated: 2nd, That in cases where a local congestion being considerable and dangerous, should require a bleeding, it should be practised with the utmost moderation, so as not to injure the general health: 3d, That we must have early recourse to tonics, and continue their use even when a local

congestion shall have rendered blood-letting necessary.—*Revue Med.*, Feb. 1833.

Observations on Periodical Diseases, by Dr. Schmidtman.—*Periodical Ophthalmia*. A young woman 28 years of age, enjoying good health, was seized towards the end of August with bilious erysipelas, which excited inflammation of the right eye. The erysipelas soon disappeared under the employment of proper means, but the face continued swollen, and the conjunctiva injected; these symptoms were combated by means of blisters and acetate of ammonia. The 7th of September the parts returned to their natural state. Nine days after I was again called on; the two eyes had been very painful from five o'clock of the afternoon; they were soon inflamed, became red and painful; these symptoms were accompanied with heat, restlessness, acceleration of the pulse, and intense thirst, without being preceded however by shivering; general sweating soon took place, and at two o'clock in the night all the constitutional symptoms disappeared. The night was tranquil, and on the following day the eye presented a slight injection, and some little pain in the angles, and along the borders of the eyelids. The digestive functions had not been disturbed, and menstruation had taken place ten days previously, at the usual time. The ophthalmia returned regularly the third day, at five o'clock precisely, continued five hours, and then disappeared to return at the end of three days. I prescribed sixteen packets of the powder of quinquina, a drachm (*un gros*) each, to take four of them the days when there was not accession. December 21st, the heat and thirst reappeared no more, and there was but slight pain, with a little injection of the conjunctiva. The quinquina was continued. The 25th of September the pain and redness of the eyes had disappeared, only on the days of the accession there was at night a little fever. Towards the middle of October these accessions also ceased, and the patient was entirely recovered. Among the number of ophthalmies which I have observed, this is the only one that assumed the periodical form. It must be very rare, for Boerhaave (*de morbis oculorum*) Platner, Heister, Juncker, and Bell, do not mention it. Frederic Hoffmann gives the name of periodical ophthalmies to those which return every year at the time of the equinox: he relates the case of a woman, who was affected every year with ophthalmia in the month of October, and who was cured by using the waters of Pyrmont. Richter is the only person who makes mention of this ophthalmia.—*Rev. Med.*, Feb. 1833.

Employment of Decoction of Belladonna in a Case of Rigidity of the Neck of the Uterus, by Dr. Ricker. A woman 19 years of age, small in size, and of a tolerably good constitution, had always menstruated regularly, though the appearance of the menses was always preceded by acute pains towards the sacrum. In the spring of 1829 she became pregnant. In the afternoon of the 1st of January, 1830, I was called on; she suffered since the preceding night

acute labour pains. The vagina was narrow and rather dry, the neck of the uterus high up, altogether obliterated; the lower part of the uterus so thick as to render it difficult to feel the head through it; the edges of the neck were a quarter of an inch thick; they were hard, and felt like cartilages. It was with difficulty we could introduce the index finger; fumigation directed towards the vulva were employed, and the application of a sponge to the neck, steeped in a decoction of chamomile. The following day, 2d January, the parts were in the same state; blood-letting to 10 ounces was practised; the contractions of the uterus were less painful, and in the evening the neck was an inch in diameter. It was natural to expect that the dilatation would continue to proceed; but the following day, 3d January, hardly any progress was made. It was decided on to daub over the neck with a mixture of hogs lard and extract of belladonna, but it all remained in the vagina, and the extract could not be carried up to the neck: then the author prepared a decoction of two drachms of the leaves of belladonna and of marsh mallow, four drachms of flaxseed; a sponge was soaked in this decoction and directed towards the neck. It was changed every half hour. At the end of three hours use of this, the neck was three inches and half in diameter; but as the head advanced very slowly, the forceps was applied at the end of some hours, and extracted a vigorous and well formed child.—*Journal de Siebold*, vol. xii. 3^e cahier.

Efficacy of Chloride of Lime in Itch, by Professor Fantonetti.—After having employed with advantage the chloride of lime in itch, in my private practice, I wished to repeat these experiments this year in the medical clinique of the University of Pavia. The result is as follows:—the medicine was employed with eight itch patients received nearly at the same period, six men and two women. The latter, the entire surface of whose body was attacked by the disease, were cured in six days. Of the six men five were almost entirely cured on the sixth or seventh day; in one only, about 16 or 18 years of age, the itch was replaced under the influence of the treatment by a general eczema, which were made to disappear under the use of hot baths; on his return home the itch reappeared, and was removed only by means of sulphurous fumigations. A younger brother of this latter patient, a child between six and seven years old, was cured very rapidly by the use of chloride of lime: at the end of fifteen days the itch having reappeared, it was entirely removed at the end of a week by the same treatment, and shewed itself a third time ten days after. After this double relapse recourse was had to sulphurous fumigations with complete success. With respect to these last two patients, I may remark, that they are both the children of a soldier of Finanza, who had a numerous family, and who certainly contracted each time anew the disease on returning to them; for none of the other patients suffered any relapse, they not being similarly circumstanced.

The dose for adults is from an ounce and a half, to two ounces in a pint (*livre*) of common water, the solution to be employed by friction

three or four times a day on the parts affected; for children the dose is one ounce in the same quantity of water. Every three days the patient is to take a warm bath for the purpose of washing and cleaning the skin, and of soothing the irritation which this application sometimes produces. It seldom happens that this method will not completely cure the itch in eight days; and I have no hesitation in asserting that this treatment is the most certain, the most prompt in its effects, and at the same time the most economical of all those usually employed for the cure of this disease.—*Annali Universali di Medicina*, Sept. 1832.

SURGERY.

Comminuted Fracture of the Lower Jaw, by Dr. Richelot.—A coachman named André, about fifty-eight years of age, of middle size, stooping behind one of his horses, received a kick in the face which threw him down. Notwithstanding the severity of the blow, he was able to get up and leave the stable in order to seek some assistance. Two medical men, who were immediately called in, were not able to stop the bleeding, which was very considerable, and contented themselves with encircling the head with a bandage applied in haste. In this state he was placed on a vehicle and carried to his master's, two leagues distant. On his arrival, his face was ecchymosed to an extraordinary degree, and so much swollen that his eyelids were strongly pressed one against the other, so that there could be nothing seen in that part but two horizontal lines, separating on either side two purple swellings. The horse's foot had struck the mouth; a portion of the upper lip, in the form of a half moon, of considerable breadth, and eight lines in depth, had been cut off, as if by a knife; almost all the teeth were torn up or shattered, some floated in his mouth, attached thereto by a long; thin shred. The maxillary bone was fractured on the left side, evidently by a counter-blow quite near the groove, which lodges the external maxillary artery. The superior maxillary bones appeared to have been pushed back, judging by the distance which separated them from the upper lip, and by the inclination backward of the superior alveolar edge. The mouth was full of blood, which had not ceased to flow from the moment the accident occurred. So great was the pain, that no attempt at reduction was made, the very introduction of the finger into the mouth being intolerable to the patient, who was in other respects a vigorous and strong man.

It was impossible to get hold of the artery which poured out so much blood; the reversion downwards and backwards of the superior alveolar edge permitted no effectual compression on the wounded lip. I supported the lower jaw with a bandage not at all tight, and I passed a few turns of a band below the nose, as well as to approxi-

mate the edges of the wound as to compress the superior labial artery, whence I supposed the hemorrhage proceeded. The night was tolerably calm; the blood ceased to flow towards morning; but the swelling of the face, and the sensibility of the parts, were still the same. The patient would not suffer any bandage to be applied, and it was with difficulty that I prevailed on him to have the lower jaw supported with a band. The first and second days succeeding the accident the patient experienced acute pains in the situation of the fracture; the pulse was raised; with respect to his intellects, they were not at all disturbed. The third day six leeches were applied to each side of the neck; they afforded some relief, and expedited the resolution of the ecchymosis which occupied the entire face. The fourth day the eyes were open; an attempt at reduction caused such acute pain, that nothing could induce the patient to allow the fragments of the broken bone to be brought into apposition. We were obliged to be satisfied with supporting the chin. As soon as the return of the hemorrhage was no longer dreaded, the inside of the mouth was cleaned with injections of warm water. Presently a clear and extremely fetid suppuration formed on different parts of the inside of the mouth, which became full every moment, a circumstance which required continual attention. For fifteen days the patient did not leave his bed, could swallow nothing, nor would he suffer any thing but a simple band under the chin, and strenuously resisted the reduction of the jaw. All his nourishment consisted in some glasses of tisan, which he caused to flow down his throat with considerable difficulty. At this time the suppuration was less abundant, less fetid, and thicker; it filled the mouth less frequently, and flowed by the nose. The wound of the upper lip was cicatrized by the approximation of its edges, and presented merely a vertical line similar to what results from the operation for hare-lip; the upper lip was slightly puckered near the nose. The pain became less violent. On this day I proceeded to the reduction of the fracture, which was supported by a rather tight bandage. In spite of my advice, the patient could not observe absolute silence, nor bring himself to make use of the œsophagus tube. He swallowed some broth by turning the head back, and pouring it into the mouth as into a funnel by the aid of a spoon. At the end of four days he could no longer support his bandage, he must have it loosened. Consolidation had already commenced, and fifteen days after this consolidation was so complete, that I was able to draw a molar tooth from the lower jaw of the right side, which caused very acute pain by pressing on the upper gum. He still nourished himself on soups, broths, &c. &c. Two months and a half after the accident, he rode on horseback, and was in good health. The anterior fragment of the jaw slipped a little under the upper fragment, which must be imputed to the perverseness of the patient; the chin must, therefore, be a little back, and yet it advances considerably before the upper jaw, which causes the superior and alveolar edges no longer to correspond. From time to time the patient

removes fragments of the necrosed bones, which are detached from the superior and inferior maxillary bones.

We may consider it surprising that so violent a blow on the face, accompanied with so much disturbance, and followed by suppuration so abundant and fetid, did not give rise to any serious symptom, and that the broken bones were so readily united, notwithstanding the motion to which they were subjected by the patient in speaking and drinking; for it must be recollected that they were not brought into apposition till the end of fifteen days, and that they were not more than four days firmly supported. The horse-shoe which had struck a part of the upper lip had come with considerable force against the corresponding jaw, as may be judged by the absence of the teeth before, by the suppuration, of a nature not at all doubtful, which poured from every part in the mouth, and by the splinters which were detached; yet no disturbance of the brain was discoverable, and this upper jaw no longer presents any traces of lesion, unless that the superior maxillary bones have been driven back with respect to the other bones of the face. Appearances are altogether favourable to this opinion, for the lower jaw is perceptibly more forward than the upper, which was not the case before the accident, and which should exist still less after the cure, by reason of the anterior fragment riding under the posterior. The upper jaw appears depressed, the cheek bones are much more prominent than they were before, and the nose is thrown to the right, the side opposite to the upper lip, which entirely changes the physiognomy of the man.—*Archiv. Gen.*, Jan. 33.

Penetrating Wound of the Abdomen—Fracture of the Os Pubis—Injury of the Bladder—Cure; by Dr. Waltz.—A young man, of vigorous constitution, somewhat more than twenty years of age, received a pistol shot, at the distance of ten paces, on the evening of the 30th August, 1827. The paleness, hippocratic countenance, and the progressive and extreme weakness of the pulse, caused us to apprehend internal hemorrhage. The ball entered at about half an inch above the penis, a little to the right, shattered the right os pubis, passed through the pelvis, and came out through the right buttock, an inch and half from the anus. Blood and urine flowed through the anterior opening of the wound. When the wounded man came a little to himself, he expressed a desire to make urine and to go to stool, without being able to accomplish it. The wounds were lightly covered, and the patient carried to the city, which was distant a league and half. On the way the tenesmus increased, the abdomen became tense and painful. Soon after his reaching the city a catheter was introduced into the bladder, and from eight to ten ounces of pure blood were removed, by which the tension of the abdomen was diminished. The catheter was left in the bladder; an injection brought on a stool. The patient would not allow the slightest touch of the wound. An anodyne was administered, and the patient slept tranquilly for some hours after midnight. Much

blood flowed through the catheter, and much urine through the anterior wound. The 31st, the discharge still continued; acute pain in the right thigh, and in the tuberosity of the ischium; towards night a little fever. 1st September; want of sleep; tenesmus; tension and sensibility of the abdomen; frequency and tension of the pulse; thirst. The catheter, which had been disturbed, being introduced again, let out three or four ounces of bloody urine, followed by thick, fetid blood. Slight remission. The catheter expelled with some degree of force; fever not very severe. 2nd September; abundant discharge of urine through the anterior wound; the posterior wound nearly closed; bowels constipated; absence of pyrexia; pain in the thigh (lenitive electuary). 3rd September; in the night, for the first time, voluntary discharge of urine; some liquid stools; appetite; some splinters of bone were taken from the wound. 4th September; considerable discharge of urine by the urethra, a little through the wound, from which some hairs and splinters were taken; general state rather satisfactory. 5th and 6th September; more abundant discharge of urine by the anterior wound; pain in the region of the fundus of the bladder, which appears hard; in the night, discharge of pus through the urethra. 7th September; a disturbed night; pains in the pelvis; discharge of urine through the anterior wound only; pulse frequent, small, and hard; features sunk, eyes dull, a feeling of sinking, loss of appetite. The catheter discovered an osseous fragment engaged in the urethra. (Emollient injections into this canal). 8th September; a good night; fever and slight pains; discharge of pus through anterior wound. (Decoct. de quinquina et elect. d'orang. comp.) 9th September; the fragment of bone has advanced to the root of the penis; excoriation of the scrotum. (The wound was surrounded with a strip of adhesive plaster, which directs the urine towards a sponge; the surrounding parts are sprinkled with powder of lycopodium). 10th–14th September; some splinters are still coming away; the wound is becoming gangrenous and secretes sanious matter. (It is dressed with pyroligneous acid.) General state less satisfactory, emaciation, loss of appetite. On the 15th September, the fragment of bones came out of the urethra, accompanied with blood and pus; pain in the pelvis; discharge of pus through the posterior wound. (Anodyne fomentations to the abdomen, stimulating injection). 16th September; the urine again flows by the urethra, followed by pus; general improvement; faces mixed with blood, and with a purulent, mucous substance. (Lime water with milk). 23rd September; urethra again obstructed by a fragment of bone, the urine flows through the wound, but subsequently it resumes its natural channel, and the wound is diminishing more and more in size. From the 1st to the 14th October several splinters of bone, and earthy concretions, were evacuated with the urine by the urethra. On the 27th October, the patient can walk a little by the aid of crutches; the right thigh is, as it were, paralysed. On the 31st October, the urethra being ob-

structed again by a splinter of bone, there is formed on the exterior wound a bladder which allows some drops of urine to escape. After these splinters were discharged the wound closed up. On the 16th November, after a debauch, felt a lumbar pain in the direction of the left ureter, which, however, soon disappeared. On the 15th April, the posterior wound again opened, and he discharged a pointed fragment of bone with some drops of urine. Cataplasms sprinkled with Goulard's wash (*eau de Goulard*) caused a rapid cicatrization of this wound. The cicatrices opened again several times, and gave issue to several splinters of bone; but, finally, after being still affected with an intermittent fever, which yielded to sulphate of quina, the patient became perfectly restored to health.—*Archiv. Gen.*, February, 1833. (*From Graefe and Walter's Journal der Chirurgie*, tom. 16, 5^e. Cahier.)

Suture of Perinæum.—M. Deneux, on the occasion of M. Roux's reading the history of a case of this nature, reproached that gentleman with going no farther back than Lamotte for the first idea of this operation: he stated that Paré, Guillemeau and Mauriceau, recommended it, and that Guillemeau was the first who put it in practice. He inveighed against the barbarous custom still existing in some parts of France of cutting down on the perinæum over the head of the child projecting at the vulva. He declared that in all cases of solution of continuity of this nature, whether natural or artificial, we must never hesitate to practise suture: He censured M. Roux for having withdrawn the needles too soon, which formerly were suffered to remain till the 24th day; in fine, he does not conceive it always necessary to place a ligature on the recto-vaginal septum. According to him it is sufficient to place them near the anus and near the vulva. M. Roux expressed his opinion that the first regular operations of this kind were owing to Lamotte, but that they had fallen into disuse, and that the twisted suture alone was employed. He thinks that reason and experience give the preference to the suture enchevillée. He then read a letter from the husband of the lady operated on, from which it appears that the recto-vaginal fistula is every day diminishing, and merely gives exit to a small quantity of gas. The general health is improving.—*Archiv. Gen.*, Nov. 1832.

Laborious Accouchement, with a Rupture of a Thrombus in the Vulva, by M. Pigeon.—In the month of October, 1831, I was called to the country to deliver a woman, who was in the third day of her labour, without any appearance of a termination. This woman was in the 44th year of her age, large, and of a good constitution, of a sanguine temperament, and habitually devoted to country employments: she was in her tenth pregnancy. The abdomen had attained an enormous size, and the waters which had escaped were, without exaggeration, estimated at more than a bucket-full. She exhibited several varices on the legs, which had inconvenienced her considerably during her latter pregnancies; and which, during the

present one, obliged her to have recourse to several bleedings. From the information taken from the person in attendance, it was ascertained that the arm and left shoulder originally presented, and that in the hope that the trunk would advance, she had not spared pulling the arm: a foolish calculation, in as much as it had none other effect than that of breaking the humerus and of entirely separating it from the shoulder. On my arrival the child had been for a considerable time dead; the state of the stump, the flaccidity of the flesh and particularly of the abdomen, the absence of pulsation in the cord, and of movements on the part of the fœtus, were so many signs manifestly proving it. For a considerable time there were no pains; the woman's strength moreover, as well as her intellect, was in the best possible state. On introducing the hand it was ascertained that the head rested by the occiput on the symphysis pubis; that the trunk was so placed, that the spine faced the right sacro-iliac symphysis and the rectum, and that the anterior part of the trunk was directed upwards and towards the right cotyloid cavity; that the left side of the trunk rested on the vagina, whilst the right was turned towards the sacro-vertebral prominence; that the trunk was very much bent, and in such a manner that the right hip resting on the sacro-vertebral prominence, the rest of the body was directed with the cord in a sort of sheath enveloping the lower extremities, and carrying them towards the right hypochondrium. Every attempt to catch hold of the feet was unavailing, besides that my fore-arm, introduced up to the elbow, could reach no further than the convexity of the right hip, and after several efforts, as far as the groove where the fingers might form a crotchet: the resistance offered by the contracted uterus was so great, that it was impossible for the hand to penetrate farther. The child then represented an oblique line from the right hypochondrium to the vagina where the left shoulder rested on the symphysis pubis. In this state of things despairing of seizing the feet, though an assistant strongly pressed back the uterus from the right flank towards the centre of the pelvis, I adopted the resolution of bringing down the buttocks, and of pushing up with the hand the stump of the engaged shoulder. This manœuvre succeeded, and with considerable ease, because the bag formed by the uterus on the right still contained some water, which having escaped by the displacement of the child, liberated this organ, and kept up a degree of suppleness in it. The introduction of a finger into the vagina of the child, after lowering the right hip, was of considerable use in these efforts, and the buttocks presented entirely, when a sudden and unforeseen accident obliged me to suspend my efforts.

During these attempts to place the child in a position favorable for its expulsion, it appears that there gave way inside the right labium of the vulva, one or two large varices, and that the blood being rapidly effused, distended it so much that it burst by a large rent. I was then in a moment of rest, the hand introduced into the uterus and the arm resting on this labium, when on looking at it by mere

chance, I saw my shirt covered with blood, and at the same time I perceived this liquid flowing on the ground, overflowing the bed, which was already filled with it. I hastened to uncover the patient to ascertain the real source of a hemorrhage so formidable, which did not take place in the uterus, and I was not a little surprised to see the blood escape by a jet as large as the little finger from this labium of the vulva, which was still the size of a large apple, and of a bluish aspect. I ceased all further attempts, particularly as the patient was for the last moment evidently becoming faint; she was terrified, pale, cold in the trunk and extremities; the pulse unequal and incapable of resistance. Permanent compression on the torn labium, which presented in the rent an oblong large clot, soon arrested this hemorrhage; it continued however without interruption for an hour; and at the end of this time the woman recovered strength; the extremities became again warm, and the pulse became hard and equal; however it continued very thready, and acquired but little strength.

True labour-pains then manifested themselves: this was to me a signal to renew my efforts. Having intrusted the compression of the labium to an intelligent assistant, I soon disengaged the buttocks, and the lower extremities, which came out, then the trunk, and the head of which the occiput rested on the symphysis pubis, was turned so that this part was opposite the right cotyloid cavity, and in this position came out without much difficulty.

The child being out, the extraction of the placenta presented some difficulties. The cord, putrified, broke on the slightest pull, so that it became necessary to introduce the hand once more into the uterus in order to effect its expulsion. This organ had already assumed the hour-glass form, and the entrance of that part which had contained the lower extremities, and the surface of which was lined by the placenta, presented a hard resisting ring, the diameter of which scarcely equalled that of a two franc piece, and was on a level with the superior aperture of the pelvis. The fingers united into the form of a cone, dilated it gradually, but slowly. It was well nigh a quarter of an hour before the hand could penetrate the interior of this portion of the uterus. The placenta adhered to it in every point: it was necessary to detach it before it could be extracted. This extraction was accompanied by the discharge of a dirty sanious liquid, which diffused a putrid odour, as did also the abdomen of the child, the epidermis of which had been removed.

Completely delivered, the patient became calm, and slept almost all the night following her delivery. The termination of the case was fortunate: by means of strict attention to diet, diluent drink, continued cataplasms over the abdomen, no new accident occurred; the rent in the right labium of the vulva was soon cicatrized, and at the end of ten days the woman was perfectly recovered.—*Mem. de l'Acad. des Sciences, Arts et Belles Lettres de Digon*, 2°. 3°. et 4°. *Livrais.*

THE
DUBLIN JOURNAL
OF
MEDICAL AND CHEMICAL SCIENCE.

1 JULY, 1833.

PART I.
ORIGINAL COMMUNICATIONS.

ART. XVI.—*On the Properties and Composition of the Compounds of Chlorine with Iodine.* By ROBERT J. KANE, M.R.I.A., Professor of Chemistry to the Apothecaries' Hall of Ireland, &c. &c.

NOTWITHSTANDING the facility with which the combinations of chlorine and iodine are formed, and the number of philosophers to whom they have been an object of study, it is curious that there still hangs over their constitution and properties a considerable shade of doubt. Immediately on the discovery of iodine, Davy and Gay Lussac succeeded in forming the body composed of five atoms of chlorine united to one of iodine, whose properties, under the name of chloro-iodic acid, or per-chloride of iodine, have been well ascertained. Both these illustrious men mentioned the existence of a sub-chloride, whose properties and composition remain still a subject of investigation, as the most approved writers give different state-

ments of the proportions in which its elements are united, and of its properties, that of depositing iodine on the addition of a base has been alone signalized.

My attention was more particularly directed to these bodies, by observing them to possess some curious properties, for which I could not at the time give any reason, their composition not being accurately known. On farther examination, the subject assumed more interest, and became so much extended, that I deemed it proper to give publicity to such results as I have hitherto arrived at. The investigation is, however, very far from being as yet complete; I am still engaged in the prosecution of the researches, and on every successive step so many new fields of enterprise come into view, that it is difficult to place any marked period to their course, or direction to their object.

PREPARATION OF THE CHLORIDES OF IODINE.

The chloride of iodine that has been long known is best prepared by taking finely divided iodine, and passing over it dried chlorine until it ceases to be absorbed. There results a white mass, soluble in a small quantity of water without decomposition, but decomposed by much water into iodic acid and chloride of hydrogen, and giving, by the action of a metallic oxide, an iodate and a chloride of the metal. This is the chloro-iodic acid of Davy. It is composed of five atoms of chlorine + 1 iodine. Its existence and composition is signalized here, as we shall have occasion frequently to refer to it; but its history having been fully developed by Davy, Dumas, and Serullas, it did not form the subject of any of our researches.

If in the preparation of the per-chloride we stop the current of chlorine before the iodine is saturated, we obtain a solid body of a brown or brownish red colour. This is the sub-chloride of iodine. It cannot be gotten pure by this process, as is proved by the uncertainty of its composition; some authors consider-

ing it to contain one atom, others two, and others again three atoms of chlorine in combination with one of iodine. To obtain this sub-chloride in a pure form, and determine its properties and composition, was my first object.

In order to obtain a compound containing the greatest possible quantity of iodine, I passed a current of chlorine through water in which iodine was diffused, leaving a considerable excess of iodine. The liquor became of a deep brownish red colour, gave off fumes of chloride of iodine highly irritant to the eyes and nose, had a peculiar smell intermediate between those of its constituents; first reddened and then bleached litmus paper; when cooled considerably, it deposited a quantity of reddish yellow matter, which was again re-dissolved by heat.

To analyse this chloride, the following process was employed:—

To a certain quantity of the saturated solution of the chloride, water of potash was added until the iodine at first precipitated was all re-dissolved. The liquor was then evaporated to dryness, the residue collected and kept at a red heat in a platinum crucible until the emission of oxygen ceased. The mass was then re-dissolved, and the liquor having been acidulated by acetic acid, nitrate of silver was added in excess. The mixed precipitate of chloride and iodide of silver was digested in ammonia, and the remaining iodide dried and weighed. The chloride was thrown down from its solution in ammonia by nitric acid, and collected.

From two analyses there were obtained—

1st. Chloride of silver . 59.5. Iodide of silver . 94.5.

2nd. 33.6. . . . 49.3.

Containing—

1st. Chlorine . . 14.68 + Iodine . . 50.97.

2nd. 8.29 + . . . 26.59.

Or in 100 parts—

	1st.	2nd.	Mean.
Chlorine .	22.36	23.76	23.06
Iodine .	77.64	76.24	76.94

But on the supposition of its being composed of an atom of each of its constituents, its composition would be in 100 parts—

Chlorine	21.87
Iodine	78.13

Now, recollecting that the ammonia used to separate the chloride from the iodide of silver, takes up a very small quantity of the latter, which is afterwards thrown down with the chloride by the nitric acid, so that by the mode of analysis, we should expect that the iodine would be given a very small quantity under, and the chlorine a corresponding quantity over the truth; and finding that such is actually the result in the analysis, it is evident that the composition of this chloride of iodine is 1 atom of chlorine + 1 atom iodine, and its atomic weight and formula are—

1 atom chlorine	35.4
1 atom iodine	126.5
1 atom chloride of iodine	161.9
And its formula	= (Ch. + I.)

PROPERTIES OF THE CHLORIDE OF IODINE.

This chloride exists in solution in water forming a deep reddish yellow solution, having a very peculiar smell resembling iodine, and highly irritating to the nose. When a drop of the solution falls on the skin, it produces a yellow stain, and corrodes the cuticle, producing pain if not washed off. When this solution is much diluted, it becomes lighter coloured, and gradually deposits iodine. When heated, it gives off iodine in considerable quantity, but much of the chloride can be distilled over unaltered; by frequent distillation, however, it may be almost totally decomposed.

With blue vegetable colours, it reacts first powerfully acid, reddening the paper, and then bleaching it. It also destroys

the green of grass, appearing to decolourize with considerable energy. When added to sulphuric acid, and the mixture cooled, a blackish matter separates, which is at first fluid owing to the increased temperature, but afterwards solidifies, and assumes a brown colour. This matter dissolves readily in water. From the liquor containing the sulphuric acid, crystals of iodic acid are occasionally deposited. When nitric acid is boiled with this chloride, a small quantity of iodic acid is deposited.

The action of chloride of iodine on oxides, is very interesting. It is well known that when mixed with an alkali, iodine is deposited, and a chloride of the metal and an iodate of the alkali formed. If more alkali be added, the precipitated iodine re-dissolves, and there are generated an iodide and iodate. The action of ammonia presents one interesting peculiarity. The first portion of water of ammonia acts as any other solution of an oxide, but when the excess of ammonia acts on the precipitated iodide, there is formed ioduret of ammonium and iodide of azote, so that when an excess of ammonia acts on chloride of iodine, a quantity of the brown fulminating compound of iodine and azote is immediately formed.

When the proto-chloride of iodine is put in contact with red oxide of mercury, oxygen gas is abundantly disengaged, and there is formed both chlorides and iodides of mercury; some iodine is also liberated.

When it is put in contact with the red or the brown oxides of lead, a considerable disengagement of gas takes place, while iodine is liberated and chloride of lead formed. With the protoxide or the peroxide of copper a similar effect is produced, but less rapidly. But with oxide of zinc the action is very violent, considerable disengagement of gas, formation of chloride of zinc, and deposition of iodine.

On oxide of silver and on oxide of arsenic the action was similar, but much less energetic. The red oxide of iron did not appear to have any action on the chloride of iodine.

From these experiments we see that when chloride of iodine

is put in contact with an oxide, a quantity of the latter is decomposed. Its metal unites with the chlorine, whilst the iodine of the chloride and the oxygen of the oxide are disengaged together, and in some cases, as where we use an oxide of an alkaline or earthy metal, unite in certain proportions forming iodic acid. It is evident that these principles meeting thus in their nascent state are in the most favourable circumstances for combining, and it is probable that some modification of these experiments may lead us to obtain the inferior degrees of oxidation of iodine, a matter to which I intend to direct my attention as soon as my other occupations will allow.

The phenomena which occur when we place chloride of iodine in contact with an iodide, are such as might have been anticipated, the chlorine combines with the most positive principle, and the iodine, as well from the iodide as from the chloride, is put at liberty; at least such has been the case in all the iodides which I submitted to experiment, of which the iodides of potassium, lead, copper, and the biniodide of mercury, may serve as examples.

One of the most interesting of the properties of the chloride of iodine is that of its action on other chlorides; and before attempting to generalize concerning the results, for indeed we have not grounds for *a priori* reasoning, we shall describe its action on such of the chlorides as I submitted to examination; and in order to render the result to which I shall immediately direct the attention of the reader more evident, I have arranged them in the form of a table.

Chlorides.	Results.
Chloride of manganese . . .	No effect.
Per-chloride of platinum . . .	Biniodate of platinum thrown down.
Proto-chloride of platinum . . .	Iodine liberated, and per-chloride of platinum formed.
Chloride of nickel	No effect.
Chloride of cobalt	No effect.
Chloride of arsenic	A splendid red liquor produced, from which, after some time, a large quantity of iodine was deposited. By distillation the liquor was found to contain some iodide of arsenic.
Chloride of barium	Precipitation of biniodate of baryta.
Proto-chloride of iron	Iodine thrown down, per-chloride of iron formed.
Per-chloride of iron	Biniodate of iron thrown down.
Proto-chloride of tin	Iodine first separated, then re-dissolved, and proto-iodide of tin formed in fine orange crystals; per-chloride of tin formed.
Per-chloride of tin	No effect.
Proto-chloride of mercury . . .	Iodine liberated, and per-chloride of mercury formed; afterwards iodide of mercury formed with excess of calomel.
Per-chloride of mercury . . .	Iodine precipitated.
Proto-chloride of antimony . . .	Iodine liberated, which forms iodide of antimony with the excess of chloride.
Proto-chloride of copper . . .	Iodine liberated, and per-chloride formed.
Per-chloride of copper	No effect.
Per-chloride of gold	No effect.

If we examine the above table of results, we shall see that the chlorides used in the experiments divide themselves into two classes, those by which iodine was separated, and those by which it was not. The latter class contains all chlorides in their highest degree of combination. They are all bodies saturated with chlorine. On the contrary, the chlorides by which iodine was separated from its combination are the—

1. Proto-chloride of iron.
2. ————— copper.
3. ————— tin.
4. ————— mercury.
5. Proto-chloride of antimony.
6. ————— platinum.
7. Chloride of arsenic.
8. Bichloride of mercury.

Of these eight chlorides six are proto-chlorides, which have a powerful tendency to unite with more chlorine and form per-chlorides. It is easy, therefore, to account for their action on

the chloride of iodine; having more affinity for chlorine than the iodine has, they unite with it, and separate the element of less energetic positive agency.

Of the action of the other two chlorides, it at first sight appears we cannot give exactly the same explanation, for chemists do not enumerate any chlorides of the respective metals containing more chlorine. It is probable, however, that there is a chloride of arsenic containing more chlorine; and as theory would lead us to the generalization of the principle, and finding it verified in so many instances, and violated apparently in only one, we may, I consider, use it as a guide in devising fresh trains of investigation. If, therefore, we consider that the general expression may be used, that when a proto-chloride acts upon a chloride of iodine, it is converted into a per-chloride with the deposition of iodine; and conversely, when a chloride separates iodine from chloride of iodine, we may consider that there exists a higher combination of the same metal with chlorine, it results that there must be a chloride of arsenic containing more chlorine than that now alone described, and that there must exist a chloride of mercury containing more chlorine than corrosive sublimate.

ON THE SECOND CHLORIDE OF IODINE.

When this proto-chloride of iodine is distilled, a considerable quantity of iodine separates; and I very often examined the distilled liquors in order to ascertain whether a determinate compound containing more chlorine could be by that process obtained. Operating in the way in which the former chloride was examined, I found the composition of the distilled chloride to be very uncertain, in different analyses obtaining the following proportional results:—

	1st.	2nd.
Chlorine	50.4	66
Iodine	126.	126

So different from each other and from any atomic relation, as to

show that the compound analyzed was not a pure chemical substance.

While examining the action of chloride of iodine on a solution of corrosive sublimate, I observed that no excess of sublimate could throw down all the iodine, and from this I thought it possible that a quantity of iodine might still remain combined, having a determinate relation to the quantity of chlorine. To ascertain this I distilled carefully the solution of chloride of iodine and corrosive sublimate (separated first from the iodine that had been deposited); very little iodine separated during the distillation, but always some; two analyses of the distilled liquor gave as a mean—

Chlorine	12.5	or	104.8
Iodine	15.1		126.5

But a compound of chlorine and iodine, in the proportions of—

3 Atoms chlorine .	(35.4) 3 = 106.2
1 Atom iodine	126.5

corresponds very nearly to this, and it is very probable that this compound really exists. However, its isolation is a matter for which a process cannot be perfectly laid down; as although on two occasions I obtained the compound giving such close results, yet it generally appeared to contain a quantity of undecomposed proto-chloride, and consequently gave much more iodine.

The properties of this compound, admitting its existence, are, generally speaking, similar to those of the proto-chloride, with one exception, which serves to distinguish it from it, and to determine when it is rendered impure by admixture with any of it. When the second chloride of iodine is mixed with proto-chloride of tin, iodine is thrown down; but when more chloride of tin is added, in place of forming the orange-red crystals of proto-iodide of tin with the excess of proto-chloride of tin, the iodine dissolves, and forms a perfectly colourless solution: therefore, in distilling the chloride of iodine, as long as the distilled liquor

forms any orange-red crystals with an excess of proto-chloride of tin, it is not sufficiently freed from proto-chloride of iodine.

When the colourless liquor formed with the second chloride of iodine is evaporated, it yields a crystalline yellow deliquescent mass, which re-dissolves in water, and contains chlorine, iodine, and tin. It is probably a combination of chloride of tin with iodide of tin, but I have not minutely examined its nature.

This second chloride of iodine may probably be found to possess other interesting properties, but I have not had sufficient time at my disposal for their development.

We thus find that in the compounds of chlorine and iodine the same series of combining proportions is discovered, as in the combinations of oxygen with chlorine, iodine, and bromine, viz., the series of odd numbers, 1, 3, 5, &c. ; for placing, for perspicuity sake, these combinations in a table, we have—

Oxygen with Chlorine.	Oxygen with Iodine.	Chlorine with Iodine.
$\begin{array}{r} \text{Oxide of Chlorine} \quad \begin{array}{r} + \quad - \\ 1 \quad + \quad 3 \end{array} \\ \text{Chloric acid} \quad \begin{array}{r} 1 \quad + \quad 5 \end{array} \\ \text{Per-chloric acid} \quad \begin{array}{r} 1 \quad + \quad 7 \end{array} \end{array}$	$\begin{array}{r} \text{Iodic acid} \quad . \quad . \quad \begin{array}{r} + \quad - \\ 1 \quad + \quad 5 \end{array} \end{array}$	$\begin{array}{r} \text{1st Chloride} \quad . \quad \begin{array}{r} + \quad - \\ 1 \quad + \quad 1 \end{array} \\ \text{2nd Chloride} \quad . \quad \begin{array}{r} 1 \quad + \quad 3 \end{array} \\ \text{Per-chloride of} \\ \text{Davy, \&c.} \quad \left. \begin{array}{l} \\ \\ \end{array} \right\} \begin{array}{r} + \quad - \\ 1 \quad + \quad 5 \end{array} \end{array}$

This establishes one more in addition to the many other common properties by which these important elementary bodies are connected.

OF THE ACTION OF IODINE ON CHLORIDES.

The iodine thrown down by a metallic proto-chloride from a solution of proto-chloride of iodine, is re-dissolved by an excess of the metallic proto-chloride, and an iodide of the metal formed. Observing this, I was led to investigate the action of iodine on chlorides in general, and I have arrived at the general expression, that when iodine is put in contact with a proto-chloride, the iodine takes away part of the metal, and the rest

is converted into per-chloride. Thus, when iodine is put in contact with proto-chloride of tin, it dissolves rapidly with the evolution of heat, and iodide of tin and per-chloride of tin are generated. The development of this result, and the examination of the apparent exceptions to it, have extended themselves so much, that I have found it necessary to defer the detailed history of that portion of the investigation to a future paper, the more so as the present one has far exceeded the limits that were at first intended for it.

ART. XVII.—*Strictures on Dr. Adams' "Observations on Mediate Auscultation as a Practical Guide in Difficult Labours."* By WILLIAM JOHNSON, M.D., Edinburgh.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL AND CHEMICAL SCIENCE.

SIR,—In the seventh number of your very excellent and valuable Journal, you have given insertion to some observations on Mediate Auscultation as a Practical Guide in Difficult Labours. As the precepts which the author of that paper wishes to inculcate, appear too dangerous to be acted upon, on slight, or perhaps, on any grounds, I solicit space in your next publication to examine the arguments on which those precepts are founded, and to point out how far they are worthy of being followed.

It is, perhaps, as well, before entering on the more immediate and most important part of the subject of his essay, to observe, that the utility of the stethoscope, so far, at least, as diseases of the chest are concerned, is no longer a matter of doubt. This length, we feel convinced, every intelligent practitioner, who has taken the trouble of becoming acquainted with the instrument, and there are few of the lately educated of the profession who will willingly avow ignorance of such a valuable part of medical education, will cordially concur in

opinion with Dr. Adams. We do, however, very much question, if with every confidence in the evidence which it affords of the morbid changes of the thoracic viscera, the profession, without *sufficient* proof, (and certainly the mere opinion of an individual cannot be received as such,) will feel inclined to attach to the stethoscope the same importance as a guide to the accoucheur, as Dr. Adams assures us he is in the habit of doing. So much we deemed necessary respecting this instrument, lest, by our denying its applicability, or, more properly, its utility in obstetric medicine, it should be supposed we intend to invalidate its use when confined to the investigation of diseases of the chest.

Let us now proceed with Dr. Adams' arguments in favour of his opinions.

"In this department," (the department of midwifery), "alone," says our author, (*Dubl. Journ.* No. vii. p. 66), "do we find ourselves so situated, that we are called upon at the moment to decide upon a practice, the consequence of which directly involves the life of a human being; in other cases symptoms and stethoscopic phenomena lend their aid to each other, but in the case to which I at present allude, we are, as it were, *altogether dependent upon the information which the stethoscope supplies to prevent us needlessly exposing the mother to the risk of a protracted labour, under the too often false idea that the child is still alive*; for, by observations drawn from the careful and repeated employment of this instrument, *we are assured of the fact that the child has ceased to live at a period long before the time to which operation was formerly deferred.*"

This quotation contains the only shadow of attempt at proving that the stethoscope affords unequivocal evidence of the death of the child; but it is objectionable, as he speaks of the observations in such a manner, that a person is led to suppose they are already quite familiar to every practical accoucheur,

while few, very few, indeed, are acquainted with them, as very few have used the stethoscope with such an object.

When I read over the passage, just quoted, I felt somewhat disappointed that Dr. Adams had neglected to enter into a detail of those stethoscopic observations on which he founds his belief in the death of the child, as soon as the pulsations of its heart become inaudible through the abdominal parietes of the mother, because I was convinced that they would, at least, have proved interesting (their utility is another question) to many of his medical brethren. On more mature consideration, however, I saw that such a detail, so far as the object which the author had in view was concerned, would have been unnecessary, and that for two very obvious reasons.

First. It is not the death of the foetus, as he wishes to enforce, that authorizes a recourse to instruments for the purpose of completing the delivery. Until the publication of the last number of the Dublin Journal, I did think that if in the whole range of midwifery there were one rule better established than another, one with which I considered every individual of the profession sufficiently acquainted, and respecting the propriety of which there was perfect unanimity, it was surely this with regard to difficult labours, that so long as the pains continue efficient, even in the slightest degree, so long as the patient's strength continues unimpaired, so long as there is no threatening of local inflammation, so long as the woman remains cool and free from all those constitutional symptoms that indicate a state of high excitement, which would be likely to terminate in that of exhaustion, the practitioner is not only not justified in interfering, but any interference on his part is considered culpable. Such being the established practice in difficult labours, and it remains with Dr. Adams to shew wherein it is erroneous, I ask of what use is the knowledge of the death of the child? I cannot forbear from quoting here a single sentence from Burns' *Midwifery* (8th edit. p. 464): "But the signs," (of the death of the child), says this distinguished author, "are in general

extremely equivocal, nor is *this much to be regretted*, for we do not operate because the child is dead, but because it is impossible for the woman to be otherwise delivered." From this, it is evident, he considers a knowledge of the death of the child no desideratum, in the art of midwifery.

Secondly. Dr. Adams' use, of the stethoscope is attended with little, if any, advantage to the accoucheur in difficult labours, for a reason the very converse of the last, namely, that whether the child be dead or alive, we do not, in fact we dare not, without incurring serious responsibility, defer the delivery after we perceive the first approaches towards a state of exhaustion, or towards some other equally untoward circumstance on the part of the mother. I need scarcely observe here, that in cases of arrest, still more in those of *locked-head*, to which Dr. Adams chiefly directs attention, swelling and tenderness of the soft parts within the pelvis, are the symptoms that most frequently indicate the necessity of prompt interference, as delay, under such circumstances, would be highly culpable.

Our choice of the means best suited for accomplishing the delivery is regulated by the circumstances of the individual case on which we are called to operate. Nor can we admit, that in this choice we are so much guided by the stethoscope as Dr. Adams, in page 72, would have us suppose. "Pursuing," says he, "this subject still farther, mediate auscultation will direct us as to the instruments necessary to be used in each peculiar case; for the forceps, an instrument which we see, even in the most experienced hands, cannot be employed without some risk, both to the mother and child, are certainly, we conceive, inapplicable when the foetal heart has ceased to be heard."

With this extract we certainly cannot concur. That embryotomy may, in the hands of Dr. Adams, be the safer operation, with respect to the mother, we do not presume to deny; but before he has recourse to such a measure in every case of difficult labour, where instrumental aid is requisite, he is bound

to prove the infallibility of his favourite guide. Nor will this be enough, so long as medical men perform embryotomy only because the woman cannot, *with safety*, be delivered by any other means. Whenever the forceps is applicable without injury to the mother, the perforator is never resorted to. I do not wish to be misunderstood, or to have it supposed that in cases where symptoms of inflammation of the soft parts within the pelvis were present or threatening, even although the forceps might conveniently enough be applied, it could be used without hazard to the mother.

These rules are so universally established by practical accoucheurs, that I have no doubt you suppose it a work of supererogation to refer to them. Had not, indeed, Dr. Adams' application of the stethoscope to this department of medicine seemed to set them aside, by making the death of the child, as ascertained by that instrument, the only indication for manual aid, and that, too, of a kind no less formidable than the perforator, I certainly do admit that I should have considered my adverting to them at such a length worse than a waste of time, inasmuch as it is occupying your pages, not with original matter, but with rules that have been too long known and too well established to be altered by the opinion of an individual. The judicious remarks of Professor Burns on the safety of the forceps, contrast very strikingly with the opinion of Dr. Adams given in the above extract.

Were I to rest satisfied with the two extracts already quoted, it would likely be inferred that they were the only objectionable passages which occur in his "Observations." I shall, therefore, be obliged to glance at two or three others, which contain the same opinions, in a form equally decisive and equally erroneous.

"In conducting a difficult labour," (I quote from page 67), we are told, "much foresight and decision are required to point out when the assistance of art may be necessary, or when we may safely depend on the efforts of unassisted nature." Now

this is all quite true ; but mark what follows. “ And in this dilemma the stethoscope affords us the most unerring guide, for by it we are enabled to pronounce with certainty on *the life or death of the fœtus*.” What, I am anxious to know, has the knowledge of the fœtus death to do with the “efforts of nature?” Has Dr. Adams never seen, even in difficult labours, the child, though still born, expelled by the natural efforts of the uterus, without the mother suffering any subsequent injury?

Acting, however, under the impression that such knowledge as, he assures us, is obtained by the stethoscope, is an indispensable acquisition to the accoucheur, he proceeds to expose the fallacy of the signs hitherto considered indicative of the child's death. His manner of doing this is so peculiar, I wish I could say happy, that it would be an act of injustice not to quote it.

“Writers on midwifery,” says he, page 67, “enumerate the following symptoms as indicating the death of the fœtus in utero : want of fœtal motion, and *pulsation at the fontanelles* ; a rolling, as of a lump or dead weight to that side on which the mother is lying ; shivering fits on the part of the mother ; flaccidity of the breasts ; fœtor of the uterine discharges ; an *emphysematous state of the scalp*, and a very loose feel of the bones of the cranium. These symptoms may assist our diagnosis before labour has commenced, but certainly are of no value once labour has set in, for every one of them may be absent and yet the child be already dead.” Now, that these symptoms are equivocal I do not deny. On the contrary, my own experience enables me to testify to their fallacy, and every author on this subject bears me out in the statement. But I should like to be informed how he ascertains *the want of pulsation at the fontanelles*, how he can discover “an *emphysematous state of the scalp*,” how he can distinguish “*the very loose feel of the bones of the cranium*,” and what object a practitioner could have in view in obtaining this information, supposing such a thing possible, before he knows whether the labour, which has not com-

menced, is to be difficult or easy ; and after its commencement, Dr. Adams gravely informs us such symptoms cannot assist our diagnosis.

In the next sentence, he says, “*But we would submit*, that the only certain criterion of deciding on the life or death of the foetus is the absence or presence of the peculiar double beat of the foetal heart, which is easily discovered by a person accustomed to the use of the stethoscope.” In the subsequent page, he continues, “If then we have once heard the foetal heart, and after a time having again instituted a most accurate examination, find that this peculiar sound is no longer audible, we have, *in my mind*, positive evidence of the death of the foetus, and that the time has arrived which not only justifies but demands the scientific use of instruments.”

Now, I ask, would Dr. Adams’ “*we would submit*,” in the one sentence, and, “*in my mind*,” in the next, be taken as a sufficient guarantee by any practitioner for the propriety of having recourse to the use of instruments, by which, be it recollected, he means the perforator. With him, however, it is not a speculative, but actually a practical, opinion ; for, says he, page 68, “of twenty-three cases of difficult labour, requiring the child’s head to be lessened, which came under my observation during the past year, the periods of labour varying from twenty-six to fifty-six hours, many of which had been several days in labour before they came under medical treatment ; in nineteen of them the foetal heart having been distinctly heard with the stethoscope, had ceased, it was *therefore neither necessary nor expedient to delay delivery*.”

That those cases did not call for the operation of embryotomy, we do not venture to say, for it appears that it was high time to interfere in some way ; but we do say, that, however called for, the operation was performed, as is no new thing in the annals of surgery, on a principle which can never be sanctioned as proper, and we have already assigned what appear to us sufficient reasons for making this statement.

We shall, however, add another. Dr. Adams cannot have been any length of time connected with such an extensive school of midwifery as the Dublin Lying-in Hospital without having witnessed, particularly after difficult labours, instances of suspended animation, where, by the use of suitable means, the child was perhaps, after much difficulty, restored. Now, I am convinced that with all his confidence in the stethoscope as an "unerring guide," he will not pretend to say that the foetal heart could be heard in such cases, at least through the abdominal parietes of the mother. Were his rules, then, that it was "therefore neither necessary nor expedient to delay delivery;" "that the time has arrived which not only justifies but demands the use of instruments," and that "when the foetal heart becomes inaudible, the forceps is inapplicable;" I say, were these rules acted on in such a case, that I leave it to himself to state the inevitable result. Nor is this an imaginary or rare case, but one of frequent occurrence. Even since I commenced writing these remarks, I met an instance of it, where, after the child was born, it was little less than a quarter of an hour before the pulsations of the heart could be perceived, and yet the child is now living and apparently healthy. I am sufficiently aware that some practical accoucheurs, of whose knowledge and judgment I entertain the very highest opinion, think that the foetus can never be resuscitated if the action of the heart cannot be felt at its birth. It is well known, however, that we have no certain criterion of death except putrefaction, and where this is wanting, I conceive, we should never omit making a trial at restoration. The best authors on this subject record cases of resuscitation, though the action of the heart could not be felt for some time after they commenced their exertions; and my own experience agrees with their accounts. But whether the heart's action, in cases of suspended animation, ceases entirely, is of comparatively little importance as to the question at issue, because every person acknowledges that it is at least very feeble, too feeble, I apprehend, to be audible by

the stethoscope before the birth of the child, and this is sufficient to bear out my objection to Dr. Adams' method of applying that instrument.

Before the publication of the last number of the *Dublin Journal of Medical and Chemical Science*, I had commenced, at the request of that excellent physician, Dr. M'Donnell of Belfast, a series of observations with the stethoscope, not certainly with the view of applying it to obstetric medicine, as Dr. Adams has done, for that was never contemplated, but with the view of obtaining an explanation of some difficulties attending the circulatory system of the foetus. I commenced my experiments on the infants soon after birth, and having become sufficiently acquainted with the sound of the foetal heart, I next extended them to the mother, both immediately before and during the period of parturition. Having now had every opportunity of using the stethoscope, I confess I am still obliged to declare that I hold the evidence of the pulsations of the foetal heart, through the abdominal parietes of the mother, extremely equivocal; and in this I do not stand alone, for many who are well acquainted with the ordinary uses of the instrument assure me, that they never could convince themselves that the foetal heart was audible previously to its birth. In several cases I did hear something, which, by allowing for the necessary indistinctness of the sound passing through the parietes of the abdomen, I fancied might be the pulsations of the foetal heart; but I have since been obliged to change my opinion, and shall state my reasons. On one occasion, I heard this sound at three different examinations more than ordinarily distinct. I counted the pulsations, and found them more frequent than the mother's pulse, which was 100, counted immediately after each examination, by from twelve to twenty beats in the minute. You may imagine my delight at the discovery, as it was one after which I frequently and zealously laboured, and contrast it with my mortification when I inform you that I had scarcely pencilled down my observations, after the last examination, till the woman was deli-

vered of a *still-born child*. Such are the results of my experiments. Let the profession view them in whatever light it thinks proper.

But I do not stop with the unsuccessful result of my own experiments, I go somewhat farther. I am sceptic enough to doubt, if, in a single instance of the nineteen cases recorded by Dr. Adams, he heard the pulsations of the foetal heart. I am far, however, from questioning by this statement his veracity. I know he believes as firmly that he has heard it in them all, as I doubt it in twice, or three times nineteen cases, I ever heard it. I consider he was deceived, and it is from his own directions to *young* practitioners, as given in page 73, that I draw this inference.

“The foetal heart, which,” says he, p. 73, “in natural presentations is generally *to be heard in the right iliac or inguinal regions, perhaps a little towards the hypogastric*, but in some cases in the opposite side of the abdomen, beats from 120 to to 160 in a minute.” Here Dr. Adams has forgotten, what is well known in Dublin, that in the foetus, as in the adult, the heart is situated in the *left*, not the *right*, side of the chest; and he does not, I feel convinced, pretend to argue, that in natural presentations the left side of the foetus is towards the right side of the mother, as must be the case were the pulsations of the foetal heart, as he very unguardedly informs us, to be heard in the *right* iliac or inguinal region. Where, may I take the liberty of inquiring of Dr. Adams, am I to look for the heart in face or anterior fontanelle presentations? According to him it would be found in the *left*, though I should surely expect to find it in the *right iliac or inguinal* region, or, more properly, perhaps, in the hypogastric, to the right of the *linea alba*.

With these remarks I take leave of Dr. Adams' *Observations on Mediate Auscultation*, in hopes that I have shewn that his directions cannot be followed without in many cases sacrificing unnecessarily the life of the child; and that admitting that the stethoscope does make us acquainted with the death of

the child *in utero*, this knowledge can be of little use, as we dare not, on that knowledge alone, in any difficult labour, have recourse to instruments for the completion of the delivery ; much less dare we, in a case where the application of the forceps was likely to be unattended with injury or danger to the mother, supersede the use of that instrument by that most horrid of all operations, embryotomy.

ART. XVIII.—*Spontaneous Human Combustion*. By EDMOND SHARKEY, M. D.

THE spontaneous combustion of the human body is a subject which has, until modern times, been regarded with incredulity by the world in general, but has ever been viewed by the medical philosopher as one of the most extraordinary in his own or any other department of physical science. “It seems,” as Dr. Mason Good has observed, “one of the most curious links in the long chain of morbid affections, and equally demands our attention as pathologists and physiologists.” Were there no stronger motive, curiosity alone would, one might suppose, be sufficient to lead an inquiring mind to the contemplation of such a subject, and yet, strange to say, very little notice has been taken of it by the medical “*savans*” of Britain, Dr. Young, in his *Med. Literat.*, and Dr. Mason Good, being the only English systematic medical writers who have adverted to it, which they have done under the title of “*Catacausis Ebriosa*.” This silence on the subject has, I suppose, arisen from the presumed unfrequency of the disease, and its consequent deficiency of practical interest ; it certainly could not from scepticism as to its existence, (though M^r Nish, in his *Anatomy of Drunkenness*, affects to deny it), for the cases on record are so numerous, and have come down to us from different countries and eras, based on such authority as renders them as authentic as any fact in

medicine. Nor (its possibility being thus substantiated) can it be affirmed that the subject is destitute of practical utility, for, as the disease may occur at any time, we know not how soon the life of an innocent individual may depend on the correctness or incorrectness of the views entertained on the point by the profession or the public at large. No case of this nature having occurred within my own experience, I will quote one or two from the writings of those who have witnessed the affection, professing the object of my present observations to be, first, to direct attention to a neglected and most interesting subject ; secondly, to bring forward some considerations respecting different theories formed to account for the phenomena which have been observed. The victims of this malady have been generally aged persons, principally females, addicted, in the majority of instances, to a long and excessive indulgence in spirituous liquors. The sources of ignition seem in some cases to have existed within themselves, in others they have been apparently external, a neighbouring stove, a lighting candle, or flash of lightning, having been considered as the origin of the combustion. In some instances the body has been found actually burnt, in others a flame has been seen flickering over it ; in others, again, a smothered heat has covertly consumed the vitals of the wretched patient. In all cases the body has been more or less completely burnt up, an oily, crumbling, sooty matter, of a horribly fetid smell, being the result. The first well authenticated account of this affection appeared, I believe, in Italy, in the form of a treatise written by the Rev. Joseph Bianchini, prebend in the city of Verona, upon the death of the Countess Cornelia Zangàri and Bandi of Cesèna. An extract from this was copied into the *Philosoph. Trans.* for the year 1744-5. It runs thus :—

“ The Countess Cornelia Bandi, in the 62nd year of her age, was all day as well as she used to be, but at night was observed, when at supper, dull and heavy. She retired, was put to bed, where she passed three hours and more in familiar dis-

courses with her maid, and in some prayers ; at last falling asleep, the door was shut. In the morning, the maid taking notice that her mistress did not awake at the usual hour, went into the bed-chamber and called her ; but not being answered, doubting of some ill accident, opened the window, and saw the corpse of her mistress in this deplorable condition : Four feet distance from the bed there was a heap of ashes, two legs untouched from the foot to the knee, with their stockings on ; between them was the lady's head, whose brains, half of the back part of the skull, and the whole chin, were burned to ashes, among which were found three fingers, blackened ; all the rest was ashes, which had this particular quality, that they left in the hand, when taken up, a greasy and stinking moisture. The air in the room was also observed cumbered with soot floating in it ; a small oil lamp on the floor was covered with ashes, but no oil in it ; two candles in candlesticks upon a table stood upright ; the cotton was left in both, but the tallow was gone and vanished ; somewhat of moisture was about the feet of the candlesticks. The bed received no damage ; the blankets and sheets were only raised on one side, as when a person rises up from it or goes in ; the whole furniture, as well as the bed, was spread over with moist and ash-coloured soot, which had penetrated into the chest of drawers even to foul the linens ; nay, the soot was also gone into a neighbouring kitchen, and hung on the walls, moveables, and utensils of it. In the room above it was, moreover, taken notice that from the lower part of the windows trickled down a greasy, loathsome, yellowish liquor, and thereabout they smelt a stink without knowing of what, and saw the soot fly around, &c." Then follow some remarks, the tenor of which is to shew that the whole was the result of a stroke of lightning, though it is acknowledged that no thunder had been heard. To this are subjoined accounts of the death of " John Hitchell, who was burned to death by lightning, and of Grace Pett, of Ipswich, whose body was consumed to a coal." I will not, however, trouble the reader with the history of more

than one other, which I will adduce, first, because it is of rather modern occurrence, and, secondly, because it contains some features bearing on the theories which have been formed respecting the nature of the process in question. For further examples, I beg leave to refer to the volume of Philos. Trans. above quoted, to the 2nd vol. of the *Acta Medica and Philos. Hafniensia*, the *Methodist Magazine* for 1869, and an article in the *Cyclopædia of Practic. Med.* by Dr. Apjohn, on "*Spontaneous Combustion*."* The other case is as follows: "A friar, named Bertholi, who lived in Mount Volere, went to the fair of Filetto, and having walked about all day, retired in the evening to the house of a relation at Fenile to spend the night. Upon his arrival he went direct to his bed-room, and had a handkerchief placed between his shoulders beneath his shirt. In a few minutes after, having been left alone, a singular noise, mingled with cries, was heard from his room; and when the people of the house rushed in, they found him on the floor surrounded by a lambent flame, which retired as they approached. When visited next morning by Joseph Battaglia, of Ponte Basio, the integuments of the right arm were found loosened from the muscles, and hanging down, and those of the back, between the shoulders and the thighs, were in the same condition. The part of the right arm which had sustained most injury appeared in a state of incipient putrefaction, and next day was quite gangrenous. On the third day there were thirst, fever, violent convulsions, fetid stools, vomiting, and delirium, and the gangrene had extended to all the injured parts. On the fourth day he fell into a comatose state, which lasted two hours, and then died. Battaglia observed during his last visit that putrefaction had commenced, and, indeed, had already made considerable progress. The nails had loosened, and were ready to fall off; the stench was insupportable, and maggots crawled from the

* Also the works of Vic d'Azyr, Lecat, Lair Kopp, Dupuytren, and Marc.

body at every point. The account which this patient gave of his singular attack was, that he felt a blow upon the right arm, as if inflicted by a club, and then saw a spark hanging to his shirt sleeve, which immediately reduced it to ashes. The handkerchief already spoken of, as also his drawers, were uninjured; but his night-cap was consumed, although his hair was not touched. There was no empyreumatic smell, and not a trace of fire or smoke in the room. It should also be observed that the night was calm, cool, and clear. A detailed account of this case appeared in the *Algemeine Literatur Zeitung*, for 1786. It is the only well established instance of an individual who suffered from such a calamity having lived sufficiently long to give an account of the manner in which he was attacked. It is highly probable that the cases recorded bear a very small proportion to those which actually occurred, and that very many of the sudden deaths with which the history of all nations abounds, and which were attributed to fulmina, were of this nature; and I cannot refrain from offering here a conjecture which has frequently occurred to my mind, though perhaps it may appear chimerical, viz., that an event of this kind is allegorically intimated in the account which mythologists have given of the death of Semele, the mother of Bacchus. It is related that she, when pregnant of Bacchus, made a request of Jupiter, her paramour, that he would visit her in all the splendor which he was wont to exhibit to his celestial peers, and that she, by reason of her mortal nature, being unable to endure the refulgence of his divine majesty, was reduced to ashes on the spot. In all this, perhaps, there is nothing more really contained, than that she, being strongly impregnated with some spirituous liquor, took fire by the mere contact of the air, for it will be remembered that Jupiter, in the heathen mythology, represented the atmosphere. If this surmise be correct, it exhibits another of the many instances in which the ancient mythologists have conveyed an account of the operations of nature under the garb of beautiful allegories. In considering this

subject, we cannot avoid being struck with astonishment when we reflect upon the incombustibility of the human body under ordinary circumstances, as appears from the great quantity of fagots necessary for the consumption of a body in the ancient practice of burning the dead, or the more recent and horrible punishment inflicted on martyrs at the stake ; and our surprise must be heightened by the other peculiarities of the phenomenon, for we are told that the most combustible articles in the neighbourhood of the sufferers, and even their wearing apparel, have remained unconsumed ; and it is further related, that the affusion of water, so far from checking, seemed to aggravate the combustion. Different theories have been devised to account for this conversion of an incombustible into a highly combustible body ; one, assuming as universal that, which, though a frequent, has yet been ascertained to be only an accidental feature in the cases recorded, attributes the combustibility of the body in this disease to its impregnation with spirit. It supposes all the tissues to be so completely saturated with the inflammable fluid, that the fumes issuing from the lungs in expiration become ignited from some neighbouring source, and that the flame thus kindled is propagated through the body. The inapplicability of this explanation to all cases contains a sufficient refutation of this theory as a general principle ; but even were it proved that an abuse of spirituous liquors had existed in every case, the change which all assimilated substances undergo, and more particularly the great affinity for water which spirituous liquors possess, render it highly improbable that they would retain their combustibility after once entering the circulation. Nor let it be said in defence of the notion, that in cases of death from extreme intoxication the different tissues have emitted a strong spirituous odour, for the presence of the principle, whatever it is, that gives the peculiar smell, does not infer that of the combustible portion ; and even if it could be shewn that the spirit entered the system pure and undiluted, it will be remembered that spirituous liquors of the greatest strength in use do not

emit combustible fumes. So much for the first origin of the combustion. Next, its continuance and propagation through the frame are grounded by them on two opposite states of the system, viz., excessive obesity or emaciation; in the former case it is affirmed that the oily substance supplies material for the combustion, in the latter, that moisture, one of the impediments to it, is removed. But in the one case, the oily substance has too great an aqueous admixture, and in the other, no degree of emaciation could procure such an absence of moisture as to allow of this explanation. In fact, I believe it might be safely affirmed, that either the fattest or the most attenuated subject might be macerated in spirits, and these set fire to without producing any thing like the phenomenon in question. And as to any confirmation of this theory sought to be derived from the similarity of the flame observed to what would result from burning spirit, there can be none, for this sort of flame is not peculiar to it, being possessed also by other substances; for instance, hydrogen gas and combinations of sulphur and carbon. Another and at present generally received theory supposes an evolution and general diffusion through the cellular tissue of hydrogen or some of its compounds; and many analogies are brought in support of it; for instance, there are on record various cases in which inflammable eructations were observed. The London Cyclopædia mentions the frequent occurrence of these in northern regions after taking immoderate quantities of gin, and quotes the case of a Bohemian peasant, in whom they were disengaged in such quantity that all efforts at extinguishing the flame proved ineffectual. The same article adduces the instance of a pig, which, upon the division of its skin, emitted a quantity of inflammable gas; likewise that of an ox which had been sick for some time previous, and from which inflammable gas proceeded in such quantity as seriously to injure the butcher and a bystander. It also mentions that two eminent anatomists were present at the dissection of a woman, from whose stomach a volume of vapour escaped, which took fire on the approach of a

candle. It has been moreover ascertained that the gases disengaged in the intestinal canal are frequently of an inflammable nature. The examples, however, above mentioned do not amount to the affection in question, the effect having been partial and confined to a single explosion, which took place external to the body. The supporters of this theory allege, that the conditions under which spontaneous combustion occurs are quite consistent with some of the most characteristic properties of hydrogen, ex. gr. that its lightness, mobility, and diffusibility, as evidenced in the phenomena of *ignes fatui* and other similar appearances, agree perfectly with the notion we must entertain of the subtile and penetrating fluid which can pervade all the tissues of the animal body. That hydrogen in combustion implicates the generality of combustible substances only when in intimate contact, and hence the escape of the clothing and adjacent furniture from a participation in the combustion; that it occurs in the old, because they being generally of a sedentary habit, and uniting debilitated constitutions with corpulence, there is an inactivity of their absorbents, which gives rise to the accumulation of a mass of combustible solid matter as well as of inflammable gas in the interstices of the body. Women, they say, are more liable than men, because their frames being more relaxed and weaker, allow a more ready diffusion of the inflammable material; for the same reason it is said that the trunk is the part most generally consumed, because the cellular tissue is there not so compact as in the head and extremities. To the obvious objection, that on this supposition an emphysematous condition should precede the catastrophe, they reply that this has been in some instances observed, and that where it has not, it may be supposed that after the combustion of what had been already evolved, a fresh supply was generated in the progress of the conflagration.

Now, in order that we may be enabled to form an adequate judgment respecting the comparative merits of these theories, to modify them, or to form any new one, it will be necessary,

first, to consider what are the conditions necessary for the production of ordinary combustion ; secondly, what determines the occurrence of spontaneous combustion when it takes place in inanimate or inorganic matter ; thirdly, what, under ordinary circumstances, draws the distinction in this respect between animal and other forms of matter ; and, fourthly, how far we are warranted by analogy in supposing that any pathological state could so far break down these barriers, so far bring the animal frame to a level with the other forms of matter, as to render it capable of undergoing a similar process. First, then, combustion takes place wherever any two forms of matter, whether simple or compound, combine chemically with the evolution of heat and light. Combustion, therefore, may result from the chemical combination of a variety of principles, but in all ordinary cases one of the substances concerned is oxygen, and the other carbon or hydrogen, or some compound of both. To these we must add, in the case of animal matter, sulphur and phosphorus. In all ordinary cases, therefore, the requisites for combustion may be set down as twofold, viz., contact with oxygen and elevated temperature. Secondly, we see instances of spontaneous combustion in inorganic matter in the firing of pyritic coal and heaps of charcoal, and in a variety of the chemical operations of the laboratory ; and, again, in dead organic matter we may refer to the spontaneous ignition of hay-stacks, and to the formidable conflagrations which have so often occurred in cotton manufactories, from the rapid absorption of oxygen gas by drying oils. What is the cause of the phenomenon in these cases ? The answer is, energetic chemical action. Thirdly, what, under ordinary circumstances, prevents a similar result in animal and vegetable bodies ? The answer is, their vitality. This principle, this undefinable principle, said, in the language of the older physiologists, to be in a state of perpetual antagonism with the laws which preside over the relations of other bodies ; this it is which prevents animals and vegetables from undergoing those changes wrought by physical laws in other

forms of matter. Fourthly, between the highest degree of vitality evidenced by the full perfection of all the individual's functions and that state which we denominate death, when the presiding vital influence withdraws its control, and leaves the beautiful animal mechanism to undergo the lot of common matter, there is an infinity of degrees constituting the long catalogue of "ills that flesh is heir to;" and we observe, that in proportion to the decrease of vitality are the inroads of physical laws. The familiar example of gangrene presents us with a local cessation of vitality, causing a spontaneous decomposition of the part affected. There is, therefore, analogy for the supposition, that a certain degree of decomposition is not incompatible with the existence of life; and, consequently, inflammable products may be generated in the body during life. But there is no necessity for having recourse to putrefaction, when we may conceive an altered secretion. What, indeed, is secretion but a decomposition, with this peculiar modification, that the vital power is the agent in its production regulating the combination of elements, and directing their union towards the generation of a certain set of products? Let us imagine, then, the alteration or deprivation of the vital power over a particular secretion, as the serous, so that instead of the normal serous exhalation into the cellular tissue, an inflammable gas were generated, would there be any thing more extraordinary in this than that the kidneys, instead of their natural secretion, should form saccharine matter? The only difference is, that in one case the new secretion is a gaseous, in the other, a liquid substance. I lately witnessed the dissection of two subjects at the Meath Hospital; in one of them, dead of empyema, there was a quantity of air in the sub-mucous tissue of the stomach, an appearance, I believe, not unfrequently found in cholera subjects. In the other, the surface of the liver was studded over with vesicles containing air. The notion of its being a vital process derives additional confirmation, I conceive, from the consideration that no instance is recorded of the dead body having been spon-

taneously consumed, at least I have not seen any such instance mentioned. The foregoing views, I think, are sufficient for enabling us to conceive how the human body may become inflammable. The next point to be considered is the first origin of the combustion. In most of the cases recorded some ignited body, as a candle, fire, or lighted pipe, has been found in the immediate vicinity of those who have suffered. But if the source of ignition appeared in all cases to be external, there would of course be an impropriety in applying the term "spontaneous" to the phenomenon. But in very many of the cases, no external source could be traced; it became, therefore, unavoidable to allow an internal or spontaneous origin. And on this point different theories have been advanced; in one, the agency of electricity was resorted to for a solution; and this electricity was referred to two different origins, some maintaining that it was generated in the body itself, and applying to it the appellation idio-electricity; others, that the communication took place by means of an accidental spark in its passage to or from the earth. Dr. Apjohn discards the former notion altogether as inadmissible in the present state of science, and therefore concludes that electricity can scarcely ever have any thing to do in producing human combustion. I naturally view with distrust any opinion of mine on this subject at issue with so high an authority as Dr. Apjohn, but cannot help thinking that the question as to the identity or affinity between galvanic and nervous influence is not so absolutely and positively determined in the negative as to warrant so hasty a rejection of an hypothesis so adequate to the solution. We know that one of the sources of electrical excitement in the external world is chemical change of form; and is there any thing irrational or unphilosophical in supposing that the operations of animal chemistry performed in the stupendous laboratory of the human body may be accompanied by a similar effect? In several of the lower animals an idio-electrical faculty resides in a visible and marked degree, nay, even in the human subject it is said to have been observed;

thus the case of a woman is recorded who possessed it to such an extent, that by the mere process of combing her hair sufficient electrical influence was excited for charging a Leyden phial. These considerations should, I think, make us at least suspend our judgment on the point. Another theory, to which Dr. Apjohn appears to give the preference, rests upon the generation, which we have reason to think sometimes does take place, of phosphuretted hydrogen in the human body. This gas is combustible by the mere contact of oxygen, independently of temperature. But this theory also seems liable to an objection which he had before raised to another, namely, the improbability of oxygen being generated within the body ; for we cannot suppose that the escape *externally* of any of this gas would do more than cause a single *external* explosion. Now, perhaps this difficulty would be obviated by the electrical theory. Blood out of the vessels, according to the experiments of Dr. Wilson Philip, disengages oxygen under the action of a galvanic current. If, then, under the depraved nervous action which I before supposed, the blood were amenable to the effects of electricity, the conditions for the combustion of this gas would be supplied. Two other theories have been advanced, both supposing an increased temperature of the body ; one of them conceiving a chemical, the other a vital process, to be the source of such increase. The one assuming, that the chemical changes which give rise to the generation of gases may at the same time produce heat sufficient for their ignition. The other rests on the increase of temperature in certain forms of disease, and a case is adduced which occurred to Balbiani, an Italian physician, which was as follows : A young farmer, labouring under intermittent fever, with gastric irritation, on the seventh day felt in the throat a burning heat ascending from the region of the stomach, so intense as to resemble the effects of the application to the body of red hot coals. His breath, which smoked, could not be borne by the hand at the distance of two feet. I will add to this a case which I saw mentioned in Bartolin's *Acta Med.*

and Philos. Hafn. It is entitled, "*Æstus Febrilis Vis Inusitata*." A pregnant woman of Orleans is stated to have been attacked by some inflammation in the hypogastric region, the heat attendant on which was so great, that the fœtus having been extracted from the uterus, its bones were found reduced to a reddish powder. Whether this is as well entitled to credit as that preceding, or whether either, I will not take on me to assert. However, without entering upon the diffuse question of the source of animal heat, I think I may venture to assert, that the truth lies in the mean between the conflicting opinions advanced on the subject, and that it is the joint result of the nervous, the respiratory, and the circulating systems. At all events, the nervous system appears to play a prominent part in regulating the animal temperature. It may, perhaps, then be worthy of consideration how far a derangement of this system might destroy the balance between the calorific and frigorific processes as to cause an accumulation of heat at all approaching that which exists in ordinary combustion, or whether there might not be generated in the all powerful alembic of the human body a compound of which we know nothing, and capable of combustion at a temperature little exceeding the ordinary animal standard. These speculations might be extended far beyond their present, I fear, inordinate length, but I will not pursue them. I cannot, however, conclude without noticing, for its whimsicality's sake, the treatment adopted by Dr. Swediaur in order to obviate any tendency to the affection. It is no other than the administration of recently voided human urine. By what analogy he was led to this practice it would be indeed difficult to divine. If I were to hazard a conjecture, I should say that he must have taken the hint from Gulliver's antiphlogistic treatment of the Lilliputian monarch's palace, with this improvement, that he extended to prevention a principle which the latter practitioner had applied solely to the cure.

ART. XIX.—*Observations on Local Blood-letting, and on some new Methods of practising it.* By JONATHAN OSBORNE, M. D., Fellow of the College of Physicians, and Physician to Sir Patrick Dun's Hospital.

FROM the earliest period local bloodletting was a practice relied on as the most immediate means of subduing inflammation. We find leeches, scarifications, and cupping in use before the time of Galen; and in some of the writings attributed to Hippocrates, bleeding is directed to be performed on various superficial veins, according as those were believed by the anatomists of the time to be connected with the affected part. During the middle ages, and afterwards, the principles of derivation and revulsion caused peculiar efficacy to be ascribed to bleeding from individual veins; and it is well known that the controversy respecting the vein to be bled in pleurisy became so violent that the partisans of the opposite theories at length invoked the interposition of the emperor Charles the Fifth. When, however, the discovery of the circulation was established, and it was proved that all the blood-vessels formed one series of continuous pipes freely communicating with each other, the importance formerly ascribed to local blood-letting was much diminished; it was believed to be matter of indifference as to the part of the system from which blood was taken, and local blood-letting appears to have fallen into comparative disuse.

When afterwards improvements were gradually introduced, and clinical institutions became the sources of practical information, the value of local blood-letting could not long remain in obscurity; and at the present time it deservedly ranks as one of the most important agents in the hands of the medical practitioner.

Blood is drawn from an artery, a vein, or from the capillaries; and while it is in the first case arterial, and in the second

Fig 5

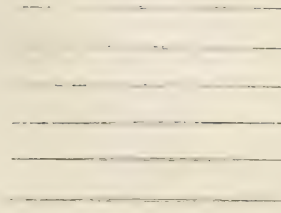


Fig 1

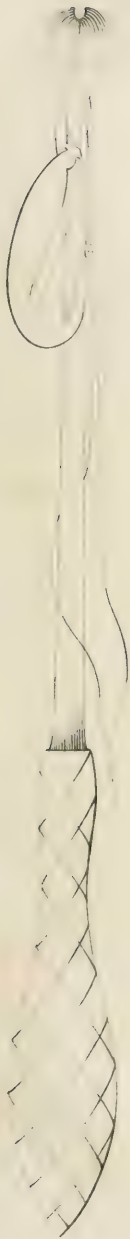


Fig 2





venous, it is in the third in an intermediate state, passing from arterial into venous. This is the state of the blood drawn by leeches and scarifications.

The temporal artery is the only one which is opened in modern times for the purpose of blood-letting; and when there is much vascular excitement in the head, as indicated by throbbing and heat, the superiority of taking blood from it above leeching is very decided, requiring, however, in order to render the benefit derived from it permanent, that cold applications should be immediately and unremittingly employed, for otherwise the sudden depletion is as suddenly succeeded by a return of the usual inordinate action. The internal carotid and its branches, which, in consequence of the exit of blood from the temporal artery, receive proportionately less blood than usual, contract, and thus continue to circulate less; and hence is produced the benefit; but this is merely transitory, unless the diminished circulation is secured by the means now alluded to.

I had often observed when a vein was opened and the blood was suffered to run along the arm in a broad stream, and exposed to a strong light, that some of the red particles appeared much darker than others. Repeated inspection shewed that there was no optical illusion; but it appeared difficult to explain how certain portions of the blood in the same vessel had been less arterialized than others. This phenomenon, however, appears evidently to derive its origin from the peculiarity in the circulation of the lungs, in which, as we learn by injections, the bronchial veins communicate with the pulmonary veins, and thus mix their venous blood with the arterial on its passage from the lungs to the left cavities of the heart. Although the blood passes from thence through the arteries and capillaries into the vein, and in its course becomes venous, yet the small portion of venous blood derived from the bronchial veins being at its first mixture much deeper in colour than the latter, the difference still continues perceptible. It is, however, hardly necessary to remark, that when the colour of the blood proceeding from a

vein is dark in consequence of obstructed respiration, this difference is much reduced, and, generally, cannot be discovered.

Opening the veins of the foot is a practice still resorted to in cases of obstructed menstruation by practitioners who must be above the influence of vulgar prejudice on the subject. The trials which I have made have not enabled me to arrive at a conclusion as to the question whether this practice possesses any advantage above general blood-letting. Bleeding from the veins of the tongue is another old practice now nearly forgotten, having been superseded by the more manageable mode of taking blood by leeches. By opening the veins on the back of the hands we can usually obtain blood with great facility when particular circumstances forbid bleeding in the arm. Bleeding from the jugular vein is not well suited for taking blood from the head, because the external jugular, which alone is within our reach, is supplied from the superficial veins of the neck, and principally from those of the larynx, but not from the interior of the head. Great benefit, however, may be derived from opening it in sudden attacks of croup.

The application of leeches is frequently a cause of great fatigue to the patient, from the length of time during which stuporing with hot water is kept up in order to promote the hæmorrhage from the leech bites; and in some cases, when this operation is continued under the bed-clothes, the damp communicated to these produces cold, and is uncomfortable to that degree as often to prohibit their use. All this is obviated by the application of warm cloths of linen or calico applied perfectly dry, and removed in succession according as they have become saturated. By these means the blood is absorbed by capillary attraction, a process which cannot take place with wet applications. When dry cloths are thus applied and renewed to cuts in the skin, or to leech bites, I have found the bleeding uniformly to continue as long as the application was kept up, it being required only to apply fresh portions of the dry cloth to

insure the continuance of capillary attraction, and thus to prevent coagulation at the mouths of the vessels.

This mode of managing leeches I am thus particular in describing, as it has enabled me to apply them in a case in which if wet cloths were used, very serious danger might arise. I allude to bronchitis, both acute and chronic, in which the application of leeches to the larynx and to the trachea in the triangular space between the mastoid muscles, has appeared to me to be the most decisive and immediately successful remedy of all those which I have ever employed. In laryngitis their utility is obvious and commonly recognized, but in bronchitis it has escaped notice, that the most immediate depletory process which can be performed on the mucous membrane of the bronchial tubes, is that of leeching the trachea and larynx. It appears to remove blood not only from the mucous membrane of that part of the bronchial tube to which the application is made, but also from the whole tract of the bronchial tubes throughout their ramifications, being nearly equally efficacious in putting an end to the cough, when the remoter tubes are affected, as when the larynx is the chief seat of disease. This application is also of singular efficacy in stopping the cough of phthisis, in so much, that by resorting to it according as required in cases in the hospital, we have been enabled to secure sleep at night, and during the day to keep the phthisical patients so free from cough that a superficial observer might readily believe that we had cured the disease.

It has been ascertained that leeches will continue to live and to draw blood although immersed in water at a temperature considerably above 100°. Now, in cases of violent inflammation of the abdominal viscera, when local abstraction of blood and warm fomentations are both at the same time imperatively demanded, as soon as leeches have been applied to the abdomen the patient may immediately be placed in a hip bath without waiting for them to fall off. Thus we may cause the relaxation and diminution of sensibility produced by the heat to

combine with the benefit to be derived from the topical loss of blood.

The application of leeches to mucous surfaces was, I believe, first described by the Surgeon-General, Mr. Crampton. Although I have not met any case of cynanche which required the direct application of leeches as advised by him, yet there can be no doubt as to the immediate benefit to be derived from it. I have resorted to the mode of applying leeches to other mucous membranes by passing a needle and thread through their tails at about one-fourth of an inch from the extremity. This practice, so far from incapacitating them from action, causes them to bite with increased ardour, and, in fact, may be used to stimulate torpid leeches. The thread to be passed through the tail of the leech should be strong, and its extremities are to be held by the operator, while, if necessary, he may direct the mouth of the leech by a probe or channel made with card to the place where his services are required.

In certain headaches confined to the frontal sinus, which, although originally derived from derangements of the digestive organs, yet do not cease when those derangements have been removed, a prompt relief is obtained from applying leeches in this manner to the interior of the nostrils; and in those cases no benefit is usually derived from leeches externally applied. The bleeding is usually rather more copious than if the application had been made on the skin; if, however, it should be deficient, the patient may encourage it by breathing over the vapour of hot water.

In inflammations of the conjunctiva, a leech thus applied to the Schneiderian membrane of the adjacent nostril evidently unloads the vessels of the eye. This application I have found of great use after the previous application of leeches to the tarsal conjunctiva. It appeared to render the improvement derived from the latter permanent, and prevented the necessity of repeating it.

In inflammations of the ear, this mode of applying a leech inside the meatus is eminently useful; and next to it in import-

ance is the application of them behind the ear as near as may be to the meatus. It may be objected, that such applications are not well suited to inflammations of the internal parts of the ear, inasmuch as those are supplied by a different set of vessels from the external. But the effect of leeches is independent of vascular connexion. For example, in inflammations of the stomach or intestinal canal the benefit derived from leeches applied to the corresponding region of the abdomen is acknowledged by all ; but the vascular connexion between those parts is as remote as that between distant regions of the body, the one being supplied from the arteries arising from the abdominal aorta, and the other from the epigastric and mammary arteries ; and that there can be no anastomosis of vessels is evident from the interposition of the peritoneum, which insulates the viscera completely from the anterior parietes of the abdomen. The same observation applies with the same force to the thoracic viscera and to the brain. In all those cases, however, the effect of local bleeding is proved so repeatedly in our daily experience, that the inability of satisfactorily explaining the way in which the effect is produced must not be allowed for one moment to press against the evidence of facts.

In inflammations of the mucous membranes of the bowels, especially of the rectum, the French practitioners apply leeches to the margin of the anus. If the leeches take externally, no benefit is derived, and to apply them internally is often difficult, on account of the violent contractions of the sphincter. Those contractions also prevent any considerable quantity of blood from being obtained from the bites. I have employed a method of taking blood from the rectum which obviates these inconveniences.

A metal rod, shaped thus, (*see Fig. 1.*), has one or more shallow grooves cut in its extremity, in which the threads attached to the tails of leeches, as above described, are inserted. The operator, holding the ends of the threads, introduces the instrument into the rectum, and pushes it up so as to cause it to

draw up the leeches along with it into the rectum. When they have thus been conveyed up beyond the sphincter, the instrument is withdrawn, and the leeches are suffered to remain till gorged with blood and loosened from their hold, when they are drawn out by means of the threads which the operator retains outside the anus.

I have never used more than four leeches at once, in this way, fearing lest too great a hæmorrhage might be produced by a greater number. From those the discharge was moderate, and was productive of that immediate benefit which was to be expected from the direct abstraction of blood from the inflamed surface. I have never witnessed any injurious effects from the leech bites, and entertain the hope that this mode of taking blood from the system of the *vena portæ* may be of use in many hepatic and intestinal diseases connected with a congested state of that system.

I am now to introduce to the reader a new instrument, designed to supersede the scarificator, and which, in order to avoid circumlocution, it is proposed to designate by the name *polytome*. In the scarificator a number of lancets are, by means of a spring, forcibly projected into the patient's body to a depth of nearly one-third of an inch. Now this depth is useless, because the cellular tissue under the spine is not vascular; and this latter is not on the average deeper than one-eighth of an inch, and the cellular and adipose substance rising up through the wound in the subsequent process of cupping choke the orifices of the vessels, and soon put a stop to their bleeding. This circumstance has not escaped the observation of Baron Larrey and others, who have in cupping abandoned the use of the scarificator, and make their incisions by means of the knife or lancet. The object of the instrument to be described, is, to improve on this practice by making a number of incisions at one stroke, of the proper depth, and no deeper, and at the same to avoid the pain and shock of the spring scarificator, which alone often forbids its use.

This instrument consists of two or more (in the present instance

six) lancets, with circular edges, fixed parallel in a frame, to which a convenient handle is attached (*Fig. 2.*) They are fixed on the frame in such a manner that they can be easily taken out for the purpose of being cleaned or whetted, and a sufficient space is left between them for wiping after ordinary use. The mode of using is to draw it rapidly along the skin, so as to make a row of continuous incisions of about one-sixteenth or one-eighth of an inch in depth, and an inch in length (*Fig. 3.*) This is attended with little or no pain, and in the case of children can be performed without their cognizance or consent by keeping the frame under the fore and two adjoining fingers, and then drawing it along in the place required. When cupping glasses are applied over those incisions, the quantity of blood which may be drawn appears to me almost without limit. That it should be far greater than that yielded by the ordinary scarificator is obvious, both on account of the orifices not being choked by the rising up of fat or cellular tissue, as already mentioned, and from the far greater number of cutaneous capillaries which are opened in those continuous superficial streaks than in the deep but short wounds inflicted by the scarificator.

Mr. Thompson, of Henry-street, in this city, has made this instrument in a very superior manner. To some he has added a guard regulated by a screw, to define the precise depth to which the blades should penetrate. In others, as in the figure, the circles at the ends of the frame serve the purpose of a guard. On using the instrument the guard will be found unnecessary, or rather disadvantageous, when it is to be applied to the hollow below the occiput, or other similar parts, and the finger is a sufficient security even when using it with the greatest rapidity. On applying it to patients who had previous experience of the scarificator, they at once acknowledged the comparative immunity from pain enjoyed under its use.

One word on cupping glasses. Those which have broad spreading edges are far inferior, both in convenience of application and in power of preserving a vacuum to those with per-

pendicular edges. In using glasses it frequently happens that the rarefaction of the air is carried too far, and the suction becomes so great as to stop the circulation of blood through the part and prevent the bleeding. This especially happens in cupping the abdomen and other soft parts.

There should always be an assortment of cupping glasses, with oval and other shaped mouths, in order to be applied as occasion may require. By attention to those particulars, cupping may be rendered of much more general application than heretofore, and in public institutions, where economy is an object, a great saving in the article of leeches may be effected.

ART. XX.—DR. O'BEIRNE *in reply to Dr. Houston on the Existence of Valves in the Rectum.*

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL AND
CHEMICAL SCIENCE.

*North Cumberland-street,
Dublin, May 28th, 1833.*

SIR,—The paper sent herewith was read at a late meeting of the Surgical Society of Ireland. I am perfectly satisfied with the opinions which it has elicited. But as my able and respected opponent has informed me of his intention to *reply* to me as soon as he conveniently can, and as the subject is important, and treated in rather a novel manner, I trust that you will see the propriety of giving the paper insertion in your Journal. With the exception of two or three verbal alterations, it is in the state in which it was read to the Society.

I have the honour to be,

Sir,

Your obedient Servant,

JAMES O'BEIRNE.

A PAPER read by Doctor Houston, at a late meeting of this society, imposes upon me the task of replying to and refuting the defence which he there makes against my published critique on his statements and opinions respecting the mucous membrane of the rectum. Fortunately, the question at issue is purely one of fact, not of opinion, and abundant materials for arriving at its satisfactory decision still remain unused by either party. I shall not, therefore, weaken the natural strength of my position by following my opponent through all the devious paths and ingenious subtleties into which he has wandered, but avail myself of these materials, and pursue a more direct, natural, and demonstrative mode of treating the subject. In the meantime, I cannot but admire the ingenuity which compels me to thus circuitously and laboriously establish points, which may be so readily decided by any one who chooses to examine the parts in question.

After quoting the description of the mucous membrane of the rectum, as I have extracted it from Dr. Knox's translation of Cloquet's "*Anatomic Descriptive*," Dr. Houston gravely charges me with selecting this extract for the purpose of protecting the character of the French anatomist, expresses his surprise that I should have selected such a passage to disprove the existence of the valves which he describes, informs us of the great disappointment which he felt in finding himself so anticipated by this anatomist, assures us "that he gave his name more with reluctance than good will, more because he was afraid of being afterwards charged with plagiarism, than that he stood in need of support from his name for the establishment of the fact in question," and concludes by boldly asserting that his statements are not disproved, as I have endeavoured to show, by the testimony of Cloquet. I confess that I was totally unprepared for such a line of defence as this, and which may be said to be at once so infinitely humorous and so perfectly unaccountable. It is infinitely humorous in making me throw the shield of my protection over the most accurate anatomist of this

or of any other age or country; and I shall now shew that it is perfectly unaccountable in supposing, even for a moment, that his description has ever been anticipated by any one, or that it is, in any respect, similar to, or reconcileable with, that given by Cloquet. Comparing both descriptions, point by point, the opposition is remarkably great. "The inner surface of the rectum," says Cloquet, "is commonly smooth in its upper half;" whereas Dr. H. asserts, that there are two valves in this portion of the membrane, one being placed at the upper end of the rectum, and another lower down. "In the lower (half)," continues Cloquet, "there are observed some parallel longitudinal wrinkles, which are thicker near the anus, and are of variable length." Dr. H., on the contrary, makes no mention of mere wrinkles, but of regular valves, and of these none in a longitudinal, but all in either a transverse or an oblique direction. "Those wrinkles," proceeds the French author, "whose number varies from four to ten or twelve, and which are called the columns of the rectum, are formed by the mucous membrane and the layer of the subjacent cellular tissue." Dr. H., on the other hand, contends that muscular fibre enters into the composition of the valves which he describes. "Between these columns," says the French anatomist, "there are almost always to be found membranous semilunar folds more or less numerous, oblique, or transverse, of which the floating edge is directed from below upwards towards the cavity of the intestine." It is on this passage that Dr. H. struggles to establish a similarity to his description: but it will only be necessary to observe, first, that each of the oblique or transverse folds described by Cloquet occupies a space intervening between two longitudinal wrinkles; secondly, that that space is necessarily very small, owing to the number of those wrinkles as compared with the diameter of the rectum; in order to see that the oblique or transverse folds which he describes bear no sort of resemblance to the valvular projections which Dr. H. describes as "occupying one-third or one-half of the circumference of the gut, and

forming a spiral track down its cavity." Lastly, Cloquet states that "these folds form a kind of lacunæ, of which the bottom is narrow and turned downwards," while Dr. H. dignifies them with the name and office of valves. It is certain, therefore, that the two descriptions are so diametrically opposite, that they do not agree even in a single point; and, consequently, that I was correct in asserting that we should reject either the one or the other.

But it is not merely the authority of Cloquet that is so directly opposed to Dr. H., for it will be found that the statements of Glisson, Morgagni, Winslow, Monro, sen., Sabatier, Portal, Sir Charles Bell, Marjolin, Green, the author of the *Dublin Dissector*, and many others, are all nearly as strongly at variance with those which he has put forth. It will be found, also, and I shall now endeavour to show, that, independently of this host of authorities, his statements can be disproved by facts drawn from different departments of medical science.

First. Fishes have a spiral arrangement of the inner coat throughout the whole of their intestinal canal, obviously for the purpose of compensating for the remarkable shortness of their intestines, as compared with the length of their bodies. But it is a fact that none of the mammalia present even the faintest rudiments of such an arrangement in any portion of their intestines. Even the orang-utang, in whom the structure of the digestive organs comes the nearest to that of man, forms no exception to the general rule. It is well known, also, that the most muscular man may pass his upper extremity, to the very axilla, up the rectum of the youngest horse, without causing hæmorrhage, or any other mischief. The force of these facts is obvious.

Secondly. Bichat, after gorging animals with food, opened them while alive, and from what he has observed, comes to the following conclusions respecting the folds of the mucous membrane of the alimentary canal. It presents," he says, "two kinds of wrinkles or folds. Those of one kind, inherent to the

structure of these membranes, are constantly met with, whatever may be the state of dilatation or contraction of the organ ; such as the pylorus, the valvulæ conniventes of the small intestines, the valve of Bauhin, &c. The other folds are, if I may say so, accidental, and are only observed during the contraction of an organ, such as those in the interior of the stomach, the large intestines, &c.”—(See *Traite des Membranes*, pp. 40, 41). Here are a number of experiments, and the highest authority, going to prove that there are no valves, or any sort of arrangement capable of acting as such, in the mucous membrane of either the cæcum, the colon, or the rectum. It is manifest, also, that Cloquet entertains the same opinion, first, because, throughout his description, he cautiously avoids using the term “valves,” but applies either the terms wrinkles, folds, or lacunæ to the irregularities of the inner membrane of the rectum ; secondly, because he distinctly states, in one of his passages, that “it is only during the empty state of the intestine that it presents irregular transverse wrinkles.” The same may be said of Marjolin, for he states that the folds in question “are effaced in proportion as the intestine is dilated.”—(*Manuel d'Anatomie*, tom. ii. p. 410). My own experiments on the healthy rectum, also, go to prove that the opposition given to the passage of instruments is owing to contraction of the muscular parietes of the intestine, and not to valves. It is perfectly clear, too, that if the ingesta are deprived of all nutritious matter by the time their remains reach the extremity of the colon, and if it be true that the rectum is not a depôt for the gradual accumulation of fecal matter, there could be no sort of use whatever in placing valves in a part of the canal so circumstanced.

Thirdly. According to Dr. H., all instruments passed up the rectum, except those of a spiral form, are calculated to produce serious injury. But, in making this assertion, how has he forgotten that the common speculum ani is straight, perfectly inflexible, and of considerable length ? How has he forgotten that the bougie, which has been so long in use for the treat-

ment of stricture in the rectum, is not only straight, but always so firm as to be but slightly flexible, and that its introduction is rarely attended with hæmorrhage or any of the other consequences which would naturally result from the destruction of regular valves? Above all, it is difficult to conceive how he has omitted to notice a circumstance mentioned by Professor Colles in the same volume of the work in which Dr. H.'s paper is inserted. The circumstance to which I allude is this: "In order," says the learned professor, "to convince my patient that his fears of stricture of the rectum were unfounded, I passed a wooden globe, three and a half inches in circumference, mounted on a stalk of whalebone, ten inches up the rectum, without having met with any obstruction."—(Dublin Hospital Reports, vol. v. p. 150). It is to be presumed, that, if Dr. H. had reflected on such facts as these, he would have seen and been convinced that straight instruments can be introduced into the rectum, not only with the most perfect safety, but with the important advantage of being more certainly directed and pressed against the point of obstruction than it is possible to accomplish by means of spiral-shaped instruments. But what are we to say of the manner in which he treats the case related by Mr. Thomas, and which I have made tell so forcibly against all his statements and opinions. He calls it "a thirty year old story about a gentleman having put his entire hand into the rectum to draw out a cane;" expresses strong doubts that any such occurrence ever took place, and roundly charges me with no little credulity in believing such a case. Such a line of observation can never be received as argument, and only shews how weak that cause must be which compels so able an advocate to adopt such a course. But what can he urge against the authority of Cruveilhier? who, in speaking of prolapsus ani, says, "I have seen sphincters permit easily the introduction of the hand." (See Dictionnaire de Medecine et de Chirurgie Pratiques. Art. Anus). The same fact is also known to every surgeon of experience. Besides what is called in veterinary medicine the pro-

cess of raking, in which the whole of the upper extremity of a man, often to the very axilla, is forced into the rectum of a horse, is, in point of fact, scarcely a less surprising instance of the great dilatability of that intestine in that animal, than Mr. Thomas's case is of that of the same intestine in man. Indeed, the dilatability of the human rectum is such, that accumulations have been known to take place in its cavity to such an extent as to fill the whole of the pelvis, and force the urinary bladder to occupy a situation in the cavity of the abdomen. A remarkable instance of this kind will be found in the *Dictionnaire des Science Medicales*, article "Constipation."

Dr. Houston considers, that the valves which he describes as being formed by the mucous membrane of the rectum may possibly become the most frequent seat of stricture in that intestine; and that "there are several facts which give probability to that conjecture." But I am totally at a loss to conceive what the facts are to which he thus alludes. Can he produce from the extensive museum to which he has so largely contributed, or from any other museum in this city, even one preparation of stricture of the rectum in which a valvular projection of the mucous membrane is seen lower down than the very highest point of the intestine? Can he even produce a specimen of stricture in the upper part of the rectum, in which it can be shewn that the stricture is formed by a thickened valve, and not by thickening of the whole of the parietes of the gut at that particular point? No, he cannot, for I have ascertained, as I have stated in my work, that none of either kind are to be found in any of the museums, schools, or hospitals of this city. Again, in cases of spasmodic stricture of the rectum, a bougie or a common candle passed to the height of nine or ten inches, will, on being withdrawn, exhibit a perfectly circular indentation at its upper extremity. Is this indentation caused by the valve which Dr. H. places at this point? Certainly not, for he describes that valve as projecting from the right wall of the gut, and as occupying but one-third or one-half of its circumference.

Such a valve would indent a bougie or a candle on the right side, and mark one-third or one-half of its circumference, but could not possibly make a circular impression. Where, then, are the facts which support Dr. H.'s conjecture, that the valves which he describes are frequently the seat of stricture? He may have recourse, perhaps, to the fact, that most authors describe strictures of the rectum as occupying very nearly the situations which he assigns to his valves. But even this circumstance will afford no support to his opinion on this point, for I have shewn that there are sources of deception in examinations of the rectum, which all these authors have overlooked, and which fully explain the otherwise singular discrepancy which exists between the sensations communicated during life, and the appearances actually presented after death.

Lastly. In cases of prolapsus ani, the mucous membrane, and frequently, also, the muscular parietes of the rectum, are inverted; and this inversion is often seen to take place to the extent of even six inches. When the mucous membrane of the gut is thus exposed to our view for such an extent, do we find, let me ask, that it exhibits any appearance whatever of valves? No, nothing more than a great number of irregular waving wrinkles. And I assert that it is not in Dr. H.'s power to produce a preparation of prolapsus ani, in which valves are visible on the surface of the tumour formed by the inverted and protruded intestine.

I have now produced from comparative anatomy, pathological anatomy, experimental physiology, and pathology, a body of facts, which, together with the testimony of Cloquet and the most distinguished anatomists, is more than sufficient to establish the justness and necessity of my critique on Dr. Houston's anatomical, physiological, and pathological views of the mucous membrane of the rectum. But as I am anxious to convince him that he has deceived himself by the method which he has selected to exhibit this membrane, I shall take the liberty of trespassing for a few minutes longer on the patience of the Society.

No parts of the human body are more visible to the naked eye, more permanent, or more constantly demonstrated without preparation of any kind, than valves. If, therefore, the valves which Dr. H. describes in the rectum really exist, and follow a spiral arrangement, they should be seen on cutting across the sigmoid flexure at its termination. But it is perfectly certain that this fair and natural mode of examination will not enable us to discover either valves or spiral arrangement, of any kind, in this portion of the mucous membrane of the intestines. In order, however, to remove all doubt on the point, by enabling the members to examine and judge for themselves, I have placed on the table of the society a bladder containing a human rectum and several inches of the sigmoid flexure of the colon. Dr. H. tells us, indeed, that his valves may be found if looked for soon after death; but I shall now shew that he has not satisfactorily answered my original question on this point, namely, why, "if they really exist, and that muscular fibres enter into their structure, they should not be discoverable at any time after death?" "My only reply to this," he says, "is, that I have placed upon the table a wet preparation made under such circumstances, which demonstrates the zig-zag or valvular state of the interior of the rectum better than any description of mine could accomplish. In this, a patient of Professor Colles's, my attempts to slit up the posterior wall of the rectum, with a probe-pointed scissors, were futile, until I piloted the tortuous channel of the gut with my finger." But if the members of the Society will only take the trouble of examining the preparation thus referred to, they will find that it is one of fistulous communication between the uterus and urinary bladder, with great morbid thickening of these organs, and, as might naturally be expected, of all the coats of the rectum, and of the whole of the surrounding cellular tissue. It is scarcely necessary to say, that the proposed question can neither be tried nor decided by a morbid preparation of this kind.

Dr. H. tells us, also, that his process of distending and

hardening the gut, and then cutting it open, is the only method of displaying his valves. But I objected to this process, as one calculated to throw the membrane into a great variety of artificial folds. In reply, he says, "it is one which I have most successfully adopted in making preparations of the membranes of the foetus, of the heart, of the intestines, of the bladder, and other hollow viscera, and which, it must be obvious, is particularly suited for making an exhibition of the walls and cavity of the rectum." But no one can know better than Dr. H. that this mode of preparing the parts which he enumerates is for the purpose of preventing them from undergoing decomposition, and not for that of exhibiting valves, for some of these parts have no valves whatever, and others have valves so permanent and fixed, that they can be seen at all times, and without any kind of preparation. It appears, however, that he has very recently discovered another mode of showing his valves, namely, by inflation and drying; and he has exhibited to the Society a specimen of the rectum so prepared, in the interior of which, I freely admit, there are several very imposing valvular projections. But it will only be necessary, in order to see how these valves were produced, to examine that specimen, and then to compare it with one which I now submit to your inspection, and the history of which is simply this: A member of the Society accidentally met with a very perfect diverticulum, or digital process, in one of the intestines of a child. In order to exhibit and preserve it, he inflated and dried a sufficient portion of the small intestines; and, during the process of drying, observed that a very large and natural-looking valve had formed at a point where the gut happened to become reflected upon itself, and at no other. This occurrence is, I find, familiar to several of my anatomical friends, and has evidently taken place in Dr. H.'s preparation, but in a much more striking manner, for it will there be seen that the surface of the gut is either indented or reflected on itself at several points, and that at each of these points there is a corresponding valve internally. But what is still more confirmatory of the fact, the same appearances

are observable in the sigmoid flexure of the colon, a situation in which neither Dr. H., nor any other anatomist, has placed valves.

Here, then, is a positive source of deception attending Dr. H.'s mode of preparation by inflation and drying, and I shall now show that his other method of proceeding is equally deceptive. "The only method," he says, "by which the condition of these valves in the distended state of the rectum is that above noticed, of filling and hardening the gut previous to being disturbed from its lateral connexions. By the ordinary procedure of distending it after removal from the body, the valves are made to disappear." In the first sentence of this passage, the circumstance of this being the only method of shewing valves in the distended state of the bowel is calculated to excite serious doubts of their existence. But has he succeeded in shewing valves in the distended state of the intestine? He has not. Every part of his account of the process, and all his preparations, show that he has cut open the gut after filling and hardening it, and that it is only after having done so that the valvular projections are seen. In the second sentence of the passage in question, he admits that by the ordinary procedure of distending the gut after removal from the body, the valves disappear. This admission is altogether fatal to his cause, for it would be a vain effort to prove that the condition of the mucous membrane of the rectum can be altered or affected in any way by the removal of that intestine from the body. In fact, it would be just as impossible to maintain such a proposition as to maintain that the removal of the stomach from the body would alter the relative condition of the lining membrane of that organ, and prevent us from observing its natural appearances. It must be manifest, therefore, that if the valves which Dr. H. describes cannot be found either in the natural or the distended state of the rectum, and that they can only be shown in its collapsed state, and by one particular process, they must be artificial, and merely the products of the process employed.

Indeed, it appears to me that their formation depends on the loose connexion between the mucous and muscular coats permitting the former to be thrown into various folds, the moment that the intestine is cut open, and that it thus rapidly passes from a state of considerable distension to one of collapse.

To conclude, I trust that this exposition of the subject at issue will convince Doctor Houston, that in consequence of placing the parts in unnatural situations, he has unknowingly produced and described appearances which do not exist in nature. But I am most desirous to remove an impression which he has received from my observations on his paper. He charges me with commenting on it in the most severe terms, yet I cannot find that my observations furnish any other grounds for the charge, than the positive and forcible language which I felt myself compelled to employ, in correcting what to me appeared, and still appear, to be important errors. I take this opportunity, however, of assuring him that, so far from feeling any kind of pleasure in assailing his paper, I have always respected him as an accomplished anatomist, and as a gentleman whose professional labours, particularly his highly original and beautiful description of the tongue of the chameleon, entitle his statements and opinions to be received with every deference consistent with the interests of humanity and science.

ART. XXI.—*Observations on the Treatment of Various Diseases*. By ROBERT J. GRAVES, M.D., M.R.I.A., King's Professor of the Institutes of Medicine.

(Continued from No. VIII. p. 173.)

FRACTURE OF A RIB PRODUCED BY A VIOLENT FIT OF COUGHING.

ON the 24th of last March, a lady residing in the country came to Dublin for the purpose of consulting me concerning a pain in her left side. She was about 47 years of age, tall and unusually strong and muscular for a female. She had a violent

fit of coughing five days before, during which she was suddenly seized with a stitch in her left side, accompanied by the sensation of something having snapped or given way. The pain was so severe, and so much affected her breath, that she obtained professional advice next day, when some leeches, and afterwards a blister, were applied, but without relief. On examining her, I found that her constitution was unaffected, her appetite being good and her pulse natural. Yet she could not make a deep inspiration without extreme pain, and she complained of great soreness and tenderness extending in every direction from central portions of the ninth and tenth ribs. When she made a very deep inspiration, the pain was felt in the situation of the left kidney, and also shot upwards to the left shoulder. These symptoms, evidently differing from those produced by either common pleurisy or pleurodyne, puzzled me not a little, and I desired the lady to remain in town in order that I might make an examination of the affected parts when she was in bed. When this was done, I found that the central point of tenderness, and that from which the pain as it were radiated, was situated not between the ribs, but on one of them, either the ninth or tenth, I think the former. It was evidently either at or very near the junction of the cartilaginous with the osseous portion of the rib.

Pressure made exactly over this spot could scarcely be tolerated, and immediately gave me the idea that the bone yielded here,—in fact, that it was broken. I mentioned my suspicion, when she told me she had mentioned to her family immediately after the accident occurred that she was sure she had broken a rib, as the feeling was similar to what she had two years before experienced on breaking one of the bones of the fore-arm. She also observed that she was much easier during the day when her stays were on, than at night when she had unlaced and taken them off. On applying a compress and roller over the part she experienced immediate relief, and gradually recovered without any other remedy being used.

There was no evidence of unusual fragility of the bones ob-

servable in this lady. The fracture of the fore-arm had been occasioned by an accident attended by the application of a sudden and violent force to the bone, and she had recovered speedily with a well ossified callus. With respect to the explanation of the fracture of a rib as an effect of coughing, it is to be recollected, that several of the abdominal muscles, which are called into action in forcible expirations and violent fits of coughing, arise from the margins of the inferior ribs; and to the agency of the unusually sudden and energetic action of some of them, we may, therefore, attribute the fracture of the rib. Altogether the case is instructive, not merely for its singularity, but because it affords a useful lesson with regard to the extreme importance of making an accurate examination of every disease before we hazard an opinion concerning its nature.

I have not read of any case similar to that just described; nor does Dupuytren, in his remarks upon the fracture of bones by the action of muscles, even allude to such an accident. He quotes the following from the *Révue Médicale*: A woman, aged 37, of a healthy constitution, was in the act of lifting a heavy weight of olives into a cart; the head and body were forcibly thrown back, to enable her to rest the weight partly on her stomach, and while making the effort to raise it higher, she felt, as it were, a crack at the middle of the sternum. This was followed by an acute pain, which forced her to press upon the part with her hands; a fracture was at once detected by the surgeon, and the lower portion of the bone was found to be projecting, and the upper one to be depressed. Compression and a bandage were applied, and in the course of a month the union was complete.*

* *Medico-Chirurgical Review*, edited by James Johnson, M.D., April, 1833, p. 532.

SUFFOCATIVE CATARRH.

Many have written on the best means of affording relief when the patient seems in danger of being suffocated by the accumulation of fluid secretions in the bronchial tubes. In such cases the secretion, instead of being scanty, is superabundant, and as long as the patient has strength, is easily expectorated. The very abundance of the secretion, however, and the constant necessity of expectoration, interferes with the function of aeration, and at length the sufferer becomes so weak that he coughs up with difficulty the sputa that obstruct the passage of air into the lungs. Each effort to do so fatigues him excessively, and adds to his debility; his countenance becomes more and more suffused and livid; the rattling of mucus is heard within the chest; the perceptive and mental faculties are dull and impaired, and, finally, the patient is suffocated after a painful and protracted struggle. This series of symptoms frequently attends common cold in the chest in those who are debilitated by great age, and is not unusual in younger persons after a severe bronchitis which has lasted until their strength has been broken, and an excessive flux from the mucous membrane of the air-passage has been the consequence of its long continuance. The late epidemic influenza, in consequence of the extreme and immediate debility, and the violent determination to the mucous membrane of the air-passages which it occasioned, was a disease peculiarly well calculated to produce the state of things above described; and, accordingly, it often terminated in suffocation, from the accumulation of mucus in the lungs. This state must be carefully distinguished in practice from the dyspnœa and tightness of chest accompanying a difficult and scanty expectoration, for stimulants are often serviceable in the former, but never in the latter. When the danger is from excess of secretion and accompanying debility, we can only attempt a cure by medicines calculated either to diminish the quantity of fluid to be expectorated, or by means adapted to increase the

patient's strength. Practitioners have sought to effect either or both of these objects by various means. Emetics, stimulating expectorants, such as decoction of polygala, with carbonate of ammonia, balsam of copaiba, combinations of antimonials, squills, and ipecacuanha, lac ammoniaci, mistura ferri composita, the frequent change of the patient's position in the bed, the inhalation of various vapours capable of stimulating the respiratory apparatus to renewed action, the application of blisters to the chest and nape of the neck, of the actual cautery along the course of the eighth pair of nerves, the use of wine or punch, have all proved occasionally successful in cases of this nature. Still, however, the instances of failure are so numerous and distressing, that it becomes the duty of every physician to seek for means still more efficacious and certain. Tonics and opium are well known to possess a powerful influence over the secretion of the bronchial tubes, and it has been long observed that when injudiciously exhibited, they often suddenly check expectoration, tighten the chest, and bring on the most formidable dyspnoea. A knowledge of those baneful effects induced me to hope that these medicines might be so managed as to relieve the affection of the chest, in which suffocation is the result of superabundant secretion and debility. As all practical men were agreed that sulphate of quinine and opium, exhibited in the usual way, had failed to produce relief in such cases, I determined to try these medicines in the form of injection. Nor was an opportunity long wanting for the trial, as in the course of a few days I was called by Mr. Wallace, of Townsend-street, to see a tradesman's wife residing at Ringsend, and who was apparently almost moribund in consequence of a violent attack of influenza. The quantity of mucus she had expectorated during the last forty-eight hours was quite enormous, and she had scarcely enjoyed a moment's repose, so perpetually was she urged by the necessity of coughing up this superabundant secretion. Within a few hours her strength had appeared quite exhausted,

her countenance became livid, and she lay with the sputa rattling in the air-passages, until the imminent danger of suffocation roused her from a state of apparent torpor to make an occasional effort to expectorate. To add to her sufferings, she had been affected with a diarrhœa during the preceding night, and her stomach was so weak that she had vomited once or twice during the morning. In this complicated and embarrassing combination of symptoms, it occurred to me, that besides applying a large blister to the nape of the neck and between the scapulæ, it would be well to use an injection of sulphate of quinine and opium for the purpose of checking the expectoration, and accordingly I directed the immediate use of an enema consisting of three ounces of solution of starch, ten grains of sulphate of quinine, and twenty drops of laudanum. I must confess my anticipations of benefit from this expedient were so slight, that I did not venture to call on our patient next day without previously ascertaining whether she was still alive, for in truth I did not expect that she could have survived the night. I was most agreeably surprised to find that she had slept very much since the administration of the lavement, and that the expectoration had remarkably diminished, while her breathing had become free, and the oppression of the chest less. Her strength had improved, and the lividity of her face disappeared; and, on the whole, her state, although not free from danger, was most satisfactory. The usual method of treating such cases was sufficient to complete her recovery.

The next person in whom I witnessed the success of the same expedient was a respectable tradesman in the vicinity of Upper Baggot-street, to whom I was called by Mr. Grant. It is needless to mention the symptoms in detail. Suffice it to say, that a combination of debility, dyspnœa, and superabundant secretion from the bronchi, formed the prominent symptoms of the case. The result was striking, and recovery rapid. In the course of a few days I had another opportunity of trying the same plan of treatment in the case of an

old woman named Jane Manning, a patient in the Meath Hospital. In her, the blueness of the lips and tongue, the extreme dyspnoea, oppression in the chest, and the rattling of mucus, which she was scarcely able to expectorate, appeared to indicate approaching dissolution ; and yet the injection of sulphate of quinine and laudanum produced effects so beneficial, that at our visit on the following day she might have been pronounced almost out of danger.

In urgent cases of this nature, emetics occasionally snatch the patient, as it were, from the jaws of death ; but they often fail ; for, as has been well remarked by Dr. Wm. Stokes, the imperfect aeration of the blood frequently so impairs the nervous energy, that emetics cannot produce their customary effect upon the stomach. In such a crisis, the practitioner may do irreparable mischief by exhibiting one emetic after another in the vain hope of exciting vomiting ; and, therefore, when he finds that the ordinary emetic powder, consisting of a scruple of ipecacuanha, and one grain of tartar emetic, has not produced the desired effect, he ought to desist from the attempt, and try other remedies. In addition to the list of those already enumerated, I beg leave to recommend a combination which I have lately used with considerable success, mustard seed and ipecacuanha. Five grains of the former in powder, with one of the latter, may be exhibited every hour, or every second hour, according to the urgency of the case. A man was evidently saved by this combination in the clinical wards of Sir Patrick Dunn's Hospital, last April, after all other remedies had failed, and at a time that his death seemed inevitable both to the pupils and myself.

A knowledge of the fact that the remedies most useful in this disease are taken from the class of stimulating expectorants capable also of producing nausea and vomiting, suggested to me the use of mustard seed. Combined with ipecacuanha, its medical qualities appear to be advantageously modified ; when it is not desirable to excite vomiting, the dose should not be re-

peated at too short intervals, but, as was before observed, in bad cases of suffocative catarrh there is frequently no danger of sickness of the stomach being produced, as was exemplified on the 17th May in the Meath Hospital, where a patient took, with temporary benefit, and without his stomach being turned, fifteen such doses of mustard seed and ipecacuanha in twenty-four hours.

To conclude, I must observe that this form of disease will often baffle the most skilful practitioner, and therefore the remedies I recommend will of course, like all others, frequently fail. An accumulation of mucous secretions in the air-passages producing the *rattles*, forms the closing scene of almost all diseases, however different in their nature. To exhibit remedies for this would be ridiculous; it is only when this accumulation is the direct consequence of actual disease attacking the air-passages themselves that we can hope for its removal. In such cases we must try every thing that experience has proved to be even occasionally useful, and must carefully watch the effect of each new medicine, for it must not be concealed that very different results are obtained from the same remedies under circumstances apparently similar. The injection of sulphate of quinine and laudanum possesses, as appears from the cases I have detailed, very great powers, and for that very reason must be used with circumspection, for if exhibited at an improper period of the disease, or in cases where expectoration is at all scanty and difficult, it may produce dangerous consequences.

MORTIFICATION OF A LARGE PORTION OF THE LIVER.

This must be an extremely rare termination of hepatic inflammation, for it is not mentioned either by Doctor Good or Andral. Doctor Mackintosh says, "mortification is, I believe, unknown as a termination of hepatitis."

Richter observes, that in no organ is gangrene so rare a consequence of inflammation as in the liver; but he quotes Frank to prove that it has been observed. On referring to Frank's description of hepatitis, and its consequences, I find it contains

no positive assertion that he had ever witnessed mortification in the liver ; he merely says, “ should gangrene have taken place, its occurrence may be conjectured from the presence of symptoms indicative of gangrene in general.” Mortification of the liver is not spoken of by Morgagni, and as to older authors, I quite agree with Dr. Mackintosh that their authority on this subject is not to be relied on, as they called every part which was dark-coloured and soft by that term. The following case, therefore, of undoubted and extensive mortification in the liver will prove, I trust, interesting to pathologists :—

Michael Brien, aged 50, was admitted into Sir Patrick Dun's Hospital on the 4th of March last. He states that he is temperate in his habits, and had been healthy until about six months ago, when having eaten a very hearty dinner, chiefly of vegetables, he was suddenly seized with violent pain in the stomach and right hypochondrium, which lasted for twenty-four hours and then subsided, leaving him, however, ever since subject to pains in the side, cramp and wind in the stomach, with some swelling of the abdomen, which last partially yielded to purgatives. On examination, we found the liver very much enlarged, and protruding far beyond its natural limits, so as to be distinctly felt forming an extensive tumour in the right and left hypochondria, and in the epigastrium. It felt hard, and was extremely painful when pressed. The patient was jaundiced, and his stools, destitute of bile, were loose and frothy, resembling barm ; the urine was deeply tinged with yellow ; he could not lie on the left side ; had no pain in either shoulder ; said that his present attack had commenced about three weeks ago, at which time the pain and swelling of the liver were observed, and were followed by œdema of the feet and legs, and ascites. His pulse are frequent, small, hard, and regular ; tongue very foul, all appetite extinguished, and he complains of great debility.

It was evident that the patient's disease was of long standing, and I concluded that his liver must have been enlarged long before the period he assigned as the commencement of his illness. The combination of enlargement of the liver, evidently

of long standing, with dropsy, emaciation, and jaundice, in a man of his age, I at once pronounced fatal. While in the hospital, his sufferings were much increased by tympanitic distention of the bowels, and from the day of his admission until his death, which took place on the 15th of March, he continued rapidly to grow weaker ; no rigors or other symptoms indicative of any peculiar termination of the hepatic inflammation occurred until six o'clock in the evening of the 14th, when, after having all the day complained of excessive abdominal pain and tenderness, and being evidently sinking, he suddenly began to vomit an extremely foetid fluid mixed with a dark grey substance. This vomiting returned at intervals until five o'clock the following morning, when he died without suffering much pain immediately previous to his death.

Post mortem examination twelve hours after death.—On opening the abdomen, about a gallon of serous fluid, deeply tinged with bile, flowed out ; the liver was greatly enlarged, extending below the umbilicus, and part of its surface was covered with recently effused lymph. Large, white, solid formations, resembling cartilage, studded its surface, and were found in its interior. They were cupped or concave on the surface, and homogeneous and consistent in their texture. By some they have been denominated *large white tubercles*, by others their nature has been compared to *scirrhus*. Between them, the texture of the liver was every where healthy. In the inferior portion of the left lobe there was an excavation larger than a man's fist, and half filled with a dark grey slough, of an extremely offensive smell, and precisely similar to the substance he had vomited. This slough was very dry, its fluids having probably been drained off by the large opening which formed a communication between the excavation in the liver and the stomach ; the sloughy appearance extended to that part of the pancreas which lay in contact with the stomach, and another perforation of the latter had been formed in this place. An evident line of demarcation existed all round the hepatic excavation, closely resembling in breadth and appearance that which

announces in external parts that nature has succeeded in arresting the progress of the disease, and is preparing for the separation of the dead from the living structure.

PURPURA HÆMORRHAGICA.

This disease is by no means unfrequent among the children of tradesmen and petty shop-keepers in Dublin, and its cause may often be traced solely to unwholesome diet, in which too much salt is habitually used. Practitioners are sometimes deceived, for on questioning the patient, he answers that his parent's circumstances are comfortable, that he has plenty to eat, and not unfrequently even gets meat for dinner. On a more accurate examination, however, it appears that the breakfast consists of bread, salt butter, and tea with very little milk; that herrings or other salted fish are often used at dinner; that bacon is also a favourite article of food, and that the supper consists of the same materials as breakfast. At dinner, too, salt, or salt butter, are used to give a relish to the potatoes; and it is only occasionally that these persons enjoy the luxury of fresh meat or vegetables, the earnings which ought to be applied to the purchase of such articles being too often expended on dram drinking. This species of diet, together with confinement and unwholesome air, renders them peculiarly liable to *purpura hæmorrhagica*, in a form which merits the name of land scurvy. My only reason for mentioning this circumstance, is, because I have seen the true cause of the disease occasionally overlooked in our hospitals, and patients subjected to different, and sometimes rather violent, plans of treatment, when all that was necessary for their cure was a nutritious dinner of fresh meat and vegetables, with milk instead of tea for breakfast. Under the influence of this change of diet, it is curious how rapidly the spots of purpura fade away, and how quickly all tendency to hæmorrhage ceases. In the meantime the pulse, which had been so frequent as to induce an inexperienced observer to adopt the idea that the disease is of a febrile nature, diminishes daily in frequency, and this under the influence of nutritious

diet, a circumstance well worthy of attention. I think that the cure is accelerated by the internal exhibition of citric acid, to the extent of half a drachm or more daily, properly diluted and sweetened.

EFFECT OF JAUNDICE UPON VISION.

The conjunctiva and some other textures of the eye, are frequently thoroughly tinged of a yellow colour in jaundice ; and yet it is rare to find that jaundiced patients exhibit any obvious derangement of the power of estimating colours. I have, however, met with three such cases, the last an intelligent girl, named Eliza Toole, who was admitted into Sir Patrick Dun's Hospital on the 18th of March, 1833. To her, white objects appeared yellow ; thus, the sheets of her bed, a white cap or handkerchief, all appeared not white, but yellow. Blue objects, on the other hand, she generally described as green, but her answers, when such objects were exposed to her view, were not always consistent. This inconsistency undoubtedly arose from her inexperience in judging of the true names of colours, for on certain occasions, as of a white handkerchief with blue spots, she at once pronounced the ground colour to be yellow and the spots green. As the same change in the colour of the humors and lens of the eye ought always to occasion the same effects, it is difficult to conceive why jaundice does not invariably produce a deception with regard to the colours of objects similar to that just described. The chief reason why no such change is observed, is because, as far as my observations go, the humors, as for instance the aqueous, escape discoloration in many cases where other fluids, e. g. the saliva, are notably yellow. It is probable that the lens also, and the vitreous humor, escape discoloration ; if it be so, it is a beautiful provision. But even when they are tinged, the alteration in the colours of objects thus produced may escape observation in most persons for the following reasons :—

First, the change takes place so gradually, that the patient perceives no sudden alteration of colour, and consequently, by

the strength of previous association of ideas, he continues to consider the colours of well known objects as unaltered. Secondly, all similar colours being tinged with the same admixture of yellow, the patient has no fixed and unadulterated standard to refer to, and therefore does not perceive the alteration in the hue of any given object. Thirdly, although the change from white to yellow must be very striking with reference to objects familiarly known to be white, such as paper, bed-linen, &c., yet when the colour in question is blue, or any of the shades of indigo, violet, or red, the yellow tint will not be obvious, for it will be blended into a new colour, compounded of yellow and that of the object viewed; it might even happen that a jaundiced person, in looking at highly coloured surfaces, would pronounce it to be white, on the same principle that Mr. Dalton has assumed in explaining why some persons are incapable of appreciating or distinguishing red colours. He supposes that those who are insensible to red light, and consequently are unable to judge of all the colours into which red light enters, labour under this defect, because they have the crystalline lens and vitreous humor of a blueish tint, which neutralizes all the red rays into white. If a blue colour of the lens and vitreous humor makes red objects appear white, then when these parts have a yellow colour, the eye must judge all objects which are of the colour complementary to yellow to be white, and must judge wrongly of the colour of all those shades into which that complementary colour enters. It becomes, therefore, a curious subject of investigation to ascertain whether any of the shades of indigo or violet appear white to a jaundiced eye, and whether other colours are not also for the same reason wrongly judged of. Of what colour does the blue of the sky appear to such persons? In some, the conjunctiva is tolerably yellow, and even the iris is tinged, at a time that the aqueous humor appears to be quite transparent, and the lens and vitreous humor are also probably uncoloured. I say probably, not

having an opportunity of dissecting such an eye, but only judging from the vision being unimpaired. This I ascertained in a gentleman I examined along with Mr. Nichol, of Dawson-street: he distinguished every colour with perfect correctness.

EMMENAGOGUES.

In the list of remedies calculated to produce the re-appearance of the natural discharge in cases of suppressed or deficient menstruation, I omitted a method of treatment long popular, and more deserving of the attention of the medical profession than other boasted specifics. This remedy is a small blister to the inside of the thigh, near the pudendum, applied a day or two before the expected period. It has been recommended by several writers, but latterly, at least in Ireland, seems to have fallen into comparative disuse. In a very obstinate case, where the discharge at each successive period was becoming more scanty, and where the other curative means I have spoken* of had failed, the blister proved eminently successful. This lady was treated by Mr. O'Flaherty, of Great Britain-street, who had the kindness to communicate to me the particulars of the case.

ON THE STATE OF THE PULSE IN HYDROCEPHALUS.

When acute hydrocephalus is not very suddenly fatal, the common observation, that it may be divided into three stages, in each of which the pulse and other symptoms undergo remarkable alterations, seems well founded. This was observable in the case of a very interesting boy named John Locke, whose body was examined in Sir Patrick Dun's Hospital in the month of April last. He died on the 15th day of the disease. As the disease commenced with violent febrile symptoms, restlessness, thirst, heat of skin, and headach, there can be no doubt that during the first days of his illness his pulse must have been

* See last number of this Journal.

quicker than natural. When admitted into hospital, on the eighth day of his illness, the pulse was 58 ; ninth day, 68 ; tenth day, 80 ; eleventh day, 92 ; twelfth day, 104 ; thirteenth day, 100 ; fourteenth day, 135 ; fifteenth day, 168. From this it appears that the pulse was probably at its minimum of frequency on the eighth day, and that it progressively rose afterwards until the last day, for the decline of four on the thirteenth day as compared with the twelfth is probably owing to some inaccuracy of observation, or to some accidental circumstance having accelerated the pulse on the twelfth day. It is interesting to observe the other symptoms, and examine what relation they bear to the frequency of the pulse.

First. When the pulse was only 58, it was irregular, and continued so for two days. After that, the irregularity began to diminish daily, and on the last day all irregularity had disappeared.

Secondly. When the pulse was 58, it was soft ; when 168, it had become sharp ; at the former period it was weak, at the latter of good strength !

Thirdly. When the pulse was at its slowest, the pupils were natural and contracted as usual. As the pulse rose, the pupils became more insensible to light, or at least became more dilated when exposed to the ordinary light of the ward, which, from the structure and position of its windows, was always pretty nearly the same at each visit. Still, though the pupils became more insensible to ordinary light as the disease advanced, yet, on the day of his death, they still contracted rapidly when exposed to the light of a candle held close to the eye. The slowness of the pulse, and the dilatation of the pupils, are attributed to increased pressure on the brain. This mode of accounting for the appearance of these symptoms in hydrocephalus is scarcely reconcileable with facts of the case before us, for had both symptoms been produced by the same cause, they would have increased or decreased together, whereas the

slowness of the pulse had ceased altogether at the time the dilatation of the pupils had commenced. Although the pupils never became absolutely insensible, yet their dilatation and sluggishness was obvious ; and on the fourteenth day of his illness, when the pulse was 135, the pupil of the left eye was more dilated than the right.

Fourthly. The convulsive fits, which form so prominent a feature in the third stage of hydrocephalus, did not take place in this case until the twelfth day, when the pulse was 104, and at a time when it is likely that the hydropic effusion had commenced. These fits affected the right side of the body rather more than the other, and the left eye was distorted, and squinted permanently, while the mouth was frequently drawn to the right side during the paroxysms. What morbid change gives rise to the fits in hydrocephalus at the very period when the occurrence of effusion would lead us to expect paralysis rather than convulsions of the muscular system ?

The following notes of the dissection of this case, for which I am indebted to Mr. Lloyd and Mr. Ryan, two gentlemen whose abilities and zeal in the observation of disease afforded me great assistance in the study of this case, will probably furnish a satisfactory answer to this question.

Post mortem examination sixteen hours after death.—On raising the calvarium, the vessels on the dura mater were found extremely turgid. On raising this membrane, the pia mater was seen greatly congested ; some effusion, tinged with blood, on the surface of the *right* hemisphere, *much greater* on the *left*, particularly toward the posterior part. The arachnoid presented a few slightly opaque spots on the upper surface of both hemispheres, and one continuous, thickened, and opaque patch on the inferior surface of the anterior lobes. On removing the cerebrum from the skull, about four ounces of serous fluid were observed in the base of the skull, and occupying the space around the medulla oblongata. The substance of the brain in

general was rather softer than natural, but not more vascular. The lateral ventricles contained about six drachms of sanguineous serum ; their lining membrane was increased in thickness and rendered semi-opaque. The *septum lucidum* had been entirely converted in a white shreddy mass which fell from its position, leaving an opening between the lateral ventricles of the size it had presented in the healthy state. This mass lay in a semi-fluid state in the anterior and lower part of the third ventricle. The *fornix* presented a remarkably soft state of its entire lower surface, *more especially at the right side*, where the softening extended through its entire thickness, and a deficiency of its margin, about three lines long, and one line in breadth, existed ; the white substance which had *fallen out of* this deficiency lay in a pulpy mass on the plexus choroides ; the surface of the velum interpositum was dotted with white matter fallen from the formix. The commissura mollis had nearly disappeared ; the superior surface of the optic thalami was soft, but this state was there extremely superficial ; *their inner surface*, to the depth of *a line*, was converted into a *blueish white jelly*. The plexus choroides was turgid, and closely studded with very minute vesicles. The white matter which has been noticed as having fallen was pulpy, and resembled white fibres connected by an extremely thin jelly.

From the above account it is clear that the vascular excitement and determination to the head had produced two series of morbid changes ; one was chiefly limited to the membranes of the brain, and ended in effusion on its surface and into its ventricles. This appears to have commenced at the very beginning of the disease, and to it may be attributed the fever, headach, vomiting, &c. of the first period, and the inclination to stupor, dilatation of the pupils, slowness of the pulse, &c. of the second period.

Had this morbid alteration been unaccompanied by any other, the pulse would never have risen, nor would convulsions

have taken place ; to account for the supervention of these remarkable symptoms, we must have recourse to the evident inflammation which had destroyed portions of the brain itself. If we suppose that the *cerebritis*, which produced the extensive softening and conversion into a pulpy structure of portions of this organ, as described in the mortem examination ; if we suppose that this *cerebritis* had commenced only at the middle period of the disease, or towards its termination, we have then no difficulty in accounting for the symptoms presented by the successive stages of the disease ; the moment the vascular action spread inwards on the substance of the brain, the moment that this new inflammation had been formed, then the pulse began to rise, and soon after convulsions, strabismus, &c. took place. I see no other way of explaining the apparently contradictory stages and symptoms which mark the course of this disease ; and, indeed, other organs supply us in abundance with analogous instances of inflammation at first occupying the membranous covering of the organ, and tending to expend itself in effusion on the surface, afterwards spreading inwards and attacking the parenchyma of the organ itself. Again, it must be recollected that as one inflammation probably commences long before the other has terminated, the symptoms at one period of the disease must be of a mixed character.

ART. XXII.—*Observations on the Form of the Rectum.* By

JOHN HOUSTON, M.D. M.R.I.A., Demonstrator of Anatomy, and Curator of the Museum in the Royal College of Surgeons in Ireland; Surgeon to Baggot-street Surgical Hospital; Surgeon to the Deaf and Dumb Institution, &c. &c.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL AND
CHEMICAL SCIENCE.

SIR,—Having this day learned from Dr. O'Beirne that he had sent for publication in your Journal a reply to a paper read by me on the 23d of March, at the Surgical Society of Ireland, in defence of a published memoir of mine on the Anatomy of the Rectum, I beg leave to send you the manuscript of that paper, though not written with a view to publication, and to request that if not too late for insertion, you will give it a place in your valuable periodical, in company with its opponent.*

I remain, Sir,

Your obedient Servant,

J. HOUSTON.

IN a work recently published by Dr. O'Beirne, entitled, "New Views of the Process of Defecation, &c.," the learned author has been pleased to comment, in the most severe terms, on an alleged anatomical inaccuracy, and a suppositious evil tendency, of a memoir published by me in the fifth volume of the Dublin Hospital Reports, respecting the conformation of the rectum;† and as the solution of the question at issue between Dr. O'Beirne and myself is very desirable, not only as being one on which there is a difference of opinion, but more espe-

* I purpose shortly to publish a more detailed account of the structure of the rectum, illustrated by drawings.

† See note in Dr. O'Beirne's work, p. 58.

cially as being one the settlement of which will have a considerable influence on the pathology and treatment of several of the diseases to which this part of the bowel is liable, I have taken the liberty of bringing forward the subject this evening, that it may receive the consideration of the Society, and be settled either in one way or the other.

In the remarks which I propose to offer, it is not my intention to make any comment on the general merits of the work published by Dr. O'Beirne, which appears to be one the result of much patient observation and labour, and the practical tendency of which will no doubt be beneficial to the public and to the profession. I shall confine myself purely to the question in dispute, which is:—What is the nature of the resistance (sometimes very considerable) given to the passage of bougies or gum elastic catheters along the canal of the rectum? Whether, according to the opinion of Dr. O'Beirne, as promulgated all through his book, the resistance is caused solely by a spasmodic action of the muscular coat of the rectum; or whether, as my memoir goes to establish, there is not in the interior of the gut a valvular arrangement of its muscular and mucous tunics, capable in many instances of giving a mechanical resistance to the progress of such instruments?

I cannot better introduce the subject to the Society than by reading the abridged heads of my essay, as given by Dr. O'B. in his work, together with his opinions and commentary thereon:

In speaking of the mode of introducing bougies up the rectum, and of the difficulty encountered in the attempt, Dr. O'Beirne says, that “hitherto the nature or cause of the increased resistance constantly met with has been quite misunderstood. White, Copeland, Shaw, Salmon, Calvert, and Colles agree in considering it to arise from the promontory or projecting ridge of the sacrum.”

“Another and a very different explanation of the occurrence has been recently offered by my friend, Dr. J. Houston, Curator of the Museum, and one of the Demonstrators in the School of the

College of Surgeons in Ireland, in a paper inserted in the fifth volume of the Dublin Hospital Reports, and entitled, ‘Observations on the Mucous Membrane of the Rectum.’ This gentleman describes the intestine as exhibiting several valvular projections of its lining membrane which have been overlooked by all the authors who have treated of diseases of the rectum, and have only been cursorily alluded to by Cloquet and some other anatomical writers. He states, that the figure of these valves is semilunar; that their surfaces are sometimes horizontal, but usually have a slightly oblique aspect; that their concave floating margins are defined, sharp, and generally directed a little upwards; that their greatest depth varies from half to three quarters of an inch and upwards in the distended state of the gut; that their structure consists of a duplicature of the mucous membrane, enclosing a few circular muscular fibres; that their number varies from two to four, but that three is the average number; that the largest and most regular is situated about three inches from the anus, the next most frequent at the upper end of the rectum, the third about midway between these, and the fourth, or that most rarely present, about one inch above the anus; and that, by being placed successively on different sides of the gut, they form a sort of spiral tract down its cavity. Considered physiologically, he conceives that these valves are necessary to support the weight of fœcal matter, and prevent its urging towards the anus, and exciting thereat a sensation demanding its discharge. Viewed in reference to disease, he considers that they explain the resistance given to the introduction of bougies; that their arrangement indicates the necessity of employing a spiral-shaped instead of a straight bougie; that they may possibly become the most frequent seat of stricture; that they have often been mistaken for strictures, and by leading to the frequent practice of bougies, may have brought on the very malady intended to be removed.”

“The whole of these statements,” says Dr. O’Beirne, “thus carefully abridged from the original, are so directly opposed

not merely to my view of the point in question, but almost to every view that I have advanced; they are so likely to be quoted against me hereafter and with effect, and they are, as I shall shew, so *unfounded in fact*, and so *injurious in practice*, that I am compelled to refute them in the fullest and most decided manner possible. To come at once therefore to the point, I do not hesitate to *deny* that the mucous membrane of the rectum exhibits *any* such valvular projections or valves as Dr. H. describes. I have sought for them repeatedly, but in vain; and I find that he is altogether incorrect in stating that Cloquet, or any anatomist, ancient or modern, makes even the slightest allusion to their existence."

The insinuation put forward by Dr. O'Beirne, that I, by a misquotation from the writings of Cloquet, have attempted to bring forward the name of that celebrated anatomist in support of what Dr. O'B. is pleased to designate incorrect statements, must appear strange to any one who will read even the extract selected by Dr. O'B. himself from the work of Cloquet. It is as follows:—

"The inner surface of the rectum is commonly smooth at its upper half, but in the lower there are observed some parallel longitudinal wrinkles, which are thicker near the anus, and are of variable length. These wrinkles, whose number varies from four to ten or twelve, and which are called columns of the rectum, are formed by the mucous membrane and the layer of the subjacent cellular tissue. Between these columns there are almost always to be found membranous semilunar folds, more or less numerous, oblique or transverse, of which the floating edge is directed from below upwards towards the cavity of the intestine. These folds form a kind of lacunæ, of which the bottom is narrow and directed downwards."

Dr. O'Beirne, in wielding this extract chosen by himself to protect the reputation of Cloquet, and to shew, as a consequence, that there was no collateral evidence in proof of my position, observes, that we have here a description remarkable

for minuteness, coming from an anatomist remarkable for the strict accuracy of his details, yet differing so widely and essentially from that given by Dr. H., also a distinguished anatomist, that we must reject either the one or the other."

An ordinary reader would have seen in the quotation above cited from Cloquet any thing but a disproof of his having borne testimony to the existence of transverse semilunar folds in the cavity of the rectum, any thing but grounds for making the description given by Cloquet the antipode of mine, any thing, in short, but reason for rejecting, as Dr. O'Beirne proposes to do, either the one description or the other.

Cloquet's account of membranous semilunar folds more or less numerous, oblique or transverse, of which the floating edge is directed upwards towards the cavity of the intestine, contains the same words, applied to the same parts, to which it was the particular object of my memoir to direct the attention of the profession.

I have said nothing of Cloquet's opinions on the subject more than that he made a "cursory allusion" to this folded condition of the rectum; and I can assure Dr. O'Beirne, that in bringing forward the name of this celebrated anatomist, I was uninfluenced by any other motive than that of giving him due credit for the accuracy of his observation. Indeed, so far from designing to adduce his name in support of a bad cause, to say the truth, I was disappointed at finding myself so anticipated by him, and gave his name more with reluctance than good will, more because I was afraid of being afterwards charged with plagiarism than that I stood in need of any support from his authority for the establishment of the fact in question.

So far, therefore, as Dr. O'B.'s argument derived from the testimony of Cloquet, is concerned, we have not found much reason for supposing that my statements were altogether "unfounded in fact."

Dr. O'Beirne proceeds to explain the cause of the difference of opinion between us on this head, by saying, "that he and

Cloquet examined the parts in the natural state, but that my mode of proceeding was any thing but natural, and nothing more or less than an attempt to exhibit natural appearances by placing the parts in an unnatural situation."

I can only account for the error here fallen into by Dr. O'Beirne respecting the unnatural character of the procedure adopted by me in the exhibition of these folds, by supposing, that my description of the process was not sufficiently explanatory, for surely no mode of preparation would be so likely to convey an accurate notion of the condition of the interior of muscular and membranous organs during the state of plenitude, as that of moderately distending them with spirits, immersing them in the same fluid for a time, and then, after they have become so hardened as to retain the form thus given them, laying them open while still immersed in the fluid. No other process can possibly effect this object so well. It is one which I have most successfully adopted in making preparations of the membranes of the foetus, of the heart, intestines, bladder, and other hollow viscera, and which, it must be obvious, is particularly suited for making an exhibition of the walls and cavity of the rectum.

That Dr. O'Beirne has fallen into a mistake on this point appears evident from the fact, that he understands and represents some sentences of my published description as a confessional evidence that the folded appearance, so strikingly marked in the preparations here exhibited, was the result of what he terms corrugation of the mucous membrane. This expedient of condemning me from my own words will be of equally little avail as that of placing Cloquet's opinion against mine, when it is understood that I have no where made use of the word corrugation, which implies puckering or throwing into folds, and that the impression conveyed by me was, that the natural folds were hardened, fixed, and preserved in their proper places, and bearing their natural shapes. This process, so far from giving rise artificially to valves in the interior of the gut, as contemplated

by Dr. O'Beirne would manifestly have a contrary tendency, instead of "causing the mucous membrane to collapse and be thrown into a variety of folds," as he states; the distention to which the parts are subjected by the fluid would rather tend to stretch and obliterate such folds, had they a previous but unsettled existence, than to develop them *de novo*.

The next part of Dr. O'Beirne's critique, aiming at a disapproval of the existence of valvular folds in the interior of the rectum, is that wherein he questions my accuracy in having stated "that their presence may be likewise ascertained in the empty state if looked for soon after death, and before the tonic contraction of the gut has subsided." My only reply to this is, that I have placed upon the table a wet preparation made under such circumstances, and which demonstrates the zigzag or valvular state of the interior of the rectum better than any description of mine could accomplish. In this, a patient of Professor Colles's, my attempts to slit up the posterior wall of the rectum with a large probe-pointed scissors were futile until I piloted the tortuous channel of the gut with my finger. The preparation is well worth the examination of the members of the Society.

Dr. O'Beirne takes particular umbrage at a phrase in my essay, which avers that the valves in the empty or contracted state of the rectum will be found to overlap each other so effectually, as to require considerable manoeuvre in conducting a bougie or the finger along the cavity of the intestine. He says, "I deny emphatically that any other manoeuvre than that, if it may be so styled, which I have described, is necessary in conducting a bougie, gum elastic tube, or finger along the cavity of the rectum." Perhaps so, indeed; and perhaps it may be *possible* to get a bougie along the canal of any rectum; but if we will examine the numerous cases reported by Dr. O'Beirne wherein the bougie was used, and attend to the accounts which he gives of the great force he employed in many instances before the instrument could be got to reach the colon, "degrees of

force which," as he expresses it, "many would think unsafe to employ," we may be inclined to believe, that if a little more manœuvre, with a proportionately less degree of force, had been used, he might have accomplished the object with equal success, and with somewhat more ease to the patient. But as Dr. O'Beirne is under an impression that the only cause for obstruction to the passage of such instruments is spasm of the muscular coat, he will accordingly endeavour to overcome that condition by force, whereas did he agree with me, as I still hope to be able to persuade him, that there is another source of difficulty arising out of the folded state of the coats of the gut, he would, I have no doubt, find benefit in his practice from acting on the suggestion.

The last proof adduced by Dr. O'Beirne of the non-existence of these disputed folds is derived from a case published by a Mr. Thomas, in which this gentleman passed his whole hand into the rectum, and extracted therefrom a piece of cane nine and a half inches long. But what does this case teach us? Not certainly to disbelieve in the presence of folds, which, though they may change the direction, do not diminish the size of the canal, but to be wary of giving full credit to extraordinary stories of this nature before we found arguments on them, and at all events, to be of opinion that the anus and rectum of this person were wonderfully dilatable. If these parts were so flaccid and yielding as to admit the entrance of a man's whole hand, we have a satisfactory explanation of the reason why a piece of cane should have found so easy ingress and egress.

I presume, after what has been said, it will appear that Dr. O'Beirne has failed in fulfilling the promise he made to his readers, "that he would refute the statements of Dr. Houston regarding the valvular arrangement of the rectum in the fullest and most decided manner possible," for it appears after all, that a queer malconstruction of Cloquet's anatomy, a confused and mistaken notion about the folds, as they appear in my preparations, being the result of artifice, a refusal to believe in the pos-

sibility of discovering them recently after death, because he never saw them under such circumstances, together with a thirty-year-old-story about a gentleman's having put his whole hand into the rectum to draw out a cane, comprise the whole stock of materials for "full and decided refutation" which Dr. O'Beirne has yet discovered.

I might, perhaps, here rest satisfied that my position respecting the existence of folds in the rectum remains unshaken by Dr. O'Beirne, and conclude my reply with the same expression of self-complacency as that with which the Doctor summed up his critique upon me, namely, "that it is unnecessary to pursue the subject further." But I deem it my duty to make known some additional facts, which I have learned since Dr. O'Beirne's critique upon my memoir has again drawn my attention to the subject, and which will tend more than arguments to establish the point in my favour.

In a middle aged male subject which I employed the other day in demonstrations on the perineum and pelvic viscera, I found that the finger, on being introduced into the rectum, encountered at a little distance from the anus a projecting ridge, which, however distinct to the feel, did not prevent the finger getting beyond it. At a short distance farther up (I cannot state the exact measurements, because the rectum had been stretched and mis-shapen by the handling it had been subjected to during demonstration) a complete barrier was discovered which could not be passed on any side by the finger, until the upper extremity of the rectum was laid hold of and pulled up so as to stretch it considerably. Then the finger, on being carried carefully round the gut, discovered a fissure or aperture along the left side, through which it found a passage onwards. This feeling of obstruction was not confined to the first trial, but was experienced at any time after whenever the stretching of the upper end of the rectum was not practised. The preparation lay in the dissecting room for days. Numbers of students saw and examined it, and Dr. McDonnell, whom I hap-

pened to meet at the College, expressed his satisfaction as to the reality of the fact as I state it. Subsequently to all this handling, and, as might be supposed, disfiguration of the interior of the intestine, I removed it from the pelvis, inflated and dried it, and have now the satisfaction of exhibiting it to the Society, as an instructive specimen of a valvular and contorted rectum.

The next subject from which I attempted to make a similar preparation was that of an individual the very opposite in every respect to the former—an aged female, emaciated to the utmost degree. This was prepared no later than yesterday, and presents, as here shewn, exactly the same form of rectum, with the same marked valvular-shaped folds, in its interior.

In my first essay respecting these shelves or valves, I stated (and I believe it was the only mis-statement I made) that the only mode by which they could be exhibited in the distended state was that of filling the gut with whiskey, previously to being disturbed from its lateral connexions. I had at the time tried no other mode, because I supposed all others had been experimented upon; but the most exaggerated notions which I then entertained respecting them, as derived from such mode of preparation, never came up to those with which I am impressed since the examination of the preparations which I here offer to the Society, made by the simple process of inflation and drying, a process in which there can be no artifice, as every one has the power of testing its efficacy.

But as no *artifice* is allowable in giving a demonstration of these folds, neither should any be practised in attempting a disproof of their existence. The rectum may, before inflation, be washed out by a stream of water let to flow through it; but it must not be over stretched, nor dissected too closely. In order that the natural condition of the tube may be exhibited, it ought, while drying, to bear as nearly as possible the form which it holds in the living body.

The only other fact which I shall for the present adduce in favour of my view of the case, is derived from a comparison

drawn by Dr. O'Beirne himself between the rectum and the œsophagus. They are both, according to him, formed of muscles of the same class, and they are both of nearly equal power and thickness, but Dr. O'Beirne will not carry the comparison so far as to assert, that they both admit, with equal readiness, of the introduction of bougies; and yet, to account for the difficulty of operating thus on the canal of the rectum, as compared with that of the œsophagus, a difficulty which is alike, whether the examination be made on the *living* or *dead* bowel, Dr. O'Beirne cannot evade the admission, that the obstacle giving rise to it must be of a mechanical nature, that, in fact, the tube of the rectum must be winding and uneven, whilst that of the œsophagus is straight and smooth.

It must be obvious, then, from the explanation I have offered of the fallacies of Dr. O'Beirne's critique on my original memoir on the subject, and still farther from the demonstration I have here given of the actual folds in question, both in the wet and in the dry state, that I have not fallen into any very serious error by having ventured on a publication of them.

I would, however, wish it to be understood, as I stated in the commencement of this paper, that I do not at all mean to undervalue the importance of the remedial plans recommended, and, as appears from his writings, successfully practised in many derangements of the bowels by Dr. O'Beirne. On the contrary, I say distinctly, that I am disposed to place much reliance on them, if exerted in moderation. I have merely, in these remarks, aimed at maintaining a position I had advanced respecting the form of the rectum, which it pleased Dr. O'Beirne to designate as not only devoid of "foundation in fact," but as fraught with much "injury" in its application to the "practice" of the profession.

ART. XXIII.—*Observations on Periostitis, Synovitis, &c.* By
 EPHRAIM M'DOWEL, M. D. M. R. I. A., one of the Surgeons
 of the Richmond Surgical Hospital, Dublin.

THE profession is greatly indebted to Mr. Brodie for his work on diseases of the joints. His classification is much more natural and scientific, and his treatment much more successful, than any that we previously possessed. But I hope to be able to communicate some further information on a form of one of these diseases, and to which he has not directed our attention ; I mean synovitis, connected more or less with periostitis.

Inflammation of the synovial membranes, not arising from injury, is well known to be frequently very acute, to run to a great height, and to cause so much constitutional disturbance, as frequently to endanger life. In the sixth case given by Mr. Brodie, we have an example of arachnitis occurring on the sudden disappearance of synovitis of the knee joint ; and his tenth and eleventh cases shew the fatal constitutional disturbance that may succeed to ulceration of even a small portion of the synovial membrane. But I shall now endeavour to shew that there are other peculiarities attendant on this affection which deserve to be noticed. Since January, 1831, I have met with several cases of acute synovitis combined with inflammation of the periosteum, to a greater or less extent, attacking different articulations with great rapidity, and causing death, in several instances, apparently by exciting pulmonary or cerebral inflammation, the affections of the periosteum and joints remaining throughout undiminished. I shall therefore endeavour to convey as clearly as possible, a knowledge of the facts as they presented themselves.

The disease did not shift from one joint to another, but continued in the articulation first affected, when another was subsequently engaged. In the cases where the periosteum was implicated, the inflammation was obviously extended to it from

the joint in which it first commenced. The affection must be more common than is generally supposed, as, within the last two years, nine examples of it have come under my observation. It is one of great severity, and runs its course to a fatal termination, in too many instances, apparently uninfluenced by the treatment ordinarily adopted in cases of inflammation of the joints, or of the fibrous structures. In every instance it was accompanied by a train of symptoms so marked, as at once to declare its nature. It occurred with and without injury, and may therefore be considered as either idiopathic or symptomatic. Its subjects were invariably young, being from ten to twenty-two years of age. In several instances they had been exposed to severe cold, and engaged in labour beyond their strength. In one instance, it was suddenly developed on the disappearance of the eruption of scarlatina maligna; there had been much cerebral excitement, and extensive inflammation, and sloughing of the mucous membrane of the fauces. On the twelfth day from the commencement of the disease, sudden pain and swelling of the right wrist joint occurred, and, in a few hours, fluctuation was evident, accompanied by considerable symptomatic fever. On the following day the left ankle joint was suddenly and similarly affected. There was almost constant screaming, till a few hours before death, which occurred on the third day from the attack of synovitis, and with symptoms indicating effusion within the cranium. And, in the ninth case to be related, synovitis of the left hip joint suddenly succeeded to acute inflammation of the fascia of the right leg. The rapidity with which the disease passed through its different stages was so various, as to occupy, in one instance, but the short period of fifty hours, and, in the others, from four to eleven days.

The local symptoms were characterized by an intensely severe pain, aggravated by the slightest pressure or movement of the affected limb, and causing frequent screaming. The tension and swelling were so considerable, that, when incisions were made to give exit to fluid supposed to exist, the divided

surfaces retracted considerably, and the swollen muscles protruded. When the inflammation was situated in the deep-seated joints, there was no discoloration of the skin, but the veins were numerous and turgid ; if the articulation was superficial, as the ankle, or when the periosteum of a bone situated near the surface was engaged, redness of the integuments occurred early, ending, as in erysipelas, abruptly. In one very acute case, roscola and urticaria were superinduced ; and, in another, a large gangrenous-looking vesicle developed itself on the inflamed surface.

The symptomatic fever was, in every instance, violent, and characterized by great depression, by a countenance anxious, flushed, and depictive of great suffering ; by moaning or screaming in a very peculiar tone, headach, restlessness, insomnia, and by more or less of delirium. In the case of a young girl, with diffused periostitis of the tibia, hydrocephalic symptoms set in three days before death, and she died in convulsions. The respiration, in every case, was short and hurried, with a slight cough, and frequently with a mucous or sonorous râle. The pulse was always rapid, varying from 100 to 180, and generally feeble. The tongue was loaded, frequently dry, and brown or red at the tip and edges, with insatiable thirst. There were frequent bilious vomiting, tenderness and fulness of the epigastrium, and always constipation, and high coloured and scanty urine. The constitutional symptoms, viewed in connexion with the local, bearing, in fact, a striking resemblance to those accompanying inflammation of the lining membrane of veins.

The morbid appearances were, in some instances, vascularity and thickening of the synovial membrane ; in others, fluid was found, with little or no change in the synovial sac. The contained matter was either healthy looking pus, or a thin, brownish red fluid, with portions of lymph. There was found ulceration of the articular capsule, and also thickening, vascularity, and, frequently, extensive detachment of the periosteum from the bone, which usually presented a peculiarly pinkish red

colour. In one case, the periosteum, for a considerable extent, was coated with a reticulated lymph, resembling that so frequently seen in pericarditis. On cutting down to the bone, purulent infiltration of the cellular tissue of the muscles was occasionally found, the muscular fibres being of a very deep red colour, and no communication existing between the matter in the muscles, and that found between the bone and its periosteum. This matter varied in its appearance, and often resembled perfectly healthy pus. In one instance, in particular, matter flowed from the femoral vein on its being divided. In the majority of the cases which I have had an opportunity of examining after death, but few morbid appearances were found within the cranium, and these consisted in effusion of a small quantity of serous fluid into the ventricles and the arachnoid sac. The brain was either soft and watery, or very firm; the pia mater was more vascular than natural, and its redness was either diffused or in patches. In one of the cases to be detailed, it will be seen that periostitis of the orbit terminated by inflammation, and suppuration of the dura mater, the arachnoid sac, and the substance of the brain. In the cavity of the thorax, morbid appearances were much more generally met with than in any other, and they may be described as consisting in recent inflammation of the pleura, with adhesions, the lymph occasionally deposited on the surface in considerable quantity, and reticulated; congestion of, and numerous small abscesses in, the lungs, more or less contiguous to the surface, containing a perfect or an imperfectly formed purulent secretion, and combined with the first stage of hepatization, and with bronchitis.

With respect to the treatment of this affection, I have found that all the remedial means recommended by authors have failed to such an extent, that only three out of eight cases terminated favourably. It should be remarked, however, that the greater number of fatal cases had been so far advanced, on admission into hospital, that the internal organs were seriously, if not irremediably, engaged. But, upon the whole, such experience as

I have had in the treatment of this disease, leads me to place more reliance upon the following plan than any other that I am acquainted with, viz., early and active depletion, early and free division of the periosteum, the introduction of mercury, so as to bring the system rapidly under its influence ; and, lastly, the energetic employment of means to support the patient during the suppurative stage.

The following was the first case that I paid particular attention to, and was an example of idiopathic synovitis and periostitis :—

Patrick Dodd, ætat. 12, admitted into the Richmond Hospital January 21, 1831. He was unable to give any account of the cause of his illness, but complained of severe pains in different parts of the body, particularly in the *left leg*, which, from the knee to the foot, is red, painful on pressure, enormously swollen and tense, with a deceptive feel of fluctuation. The right hip joint and thigh are also swollen, painful, and very tense, the veins much distended, the countenance pale, sunken, and exhausted, expressive of much suffering and anxiety ; tongue dry and brown ; pulse rapid and very feeble ; complete prostration of strength. The fascia of the leg was divided ; the edges of the incision retracted considerably, and the muscles protruded ; but no purulent matter was discharged. Warm drinks, heat to the feet, an emollient enema, and carbonate of ammonia with camphor mixture.

22nd. Re-action has taken place ; tongue loaded ; much thirst ; the tension of the leg being still very great, two incisions were made through the fascia on its inner side, but without giving exit to pus ; a bolus of calomel, followed in six hours with a draught of castor oil ; poultices and fomentations.

23rd. No sleep ; almost constant moaning and raving ; urine and feces passed involuntarily ; tongue dry and brown ; pulse 140, and feeble. The erysipelatous redness has extended from the left leg far up on the thigh, and is of a much deeper hue than before. The tension of the right thigh has so much

increased, that an incision of considerable length was made along its outer side, the fascia instantly flew open, presenting a sharp hard edge, but no fluid was given exit to ; head to be shaved, and cold lotion applied ; diaphoretic mixture and saline draughts ; leeches to the foot and thigh.

24th. Pulse small, feeble, and rapid ; countenance more sunk ; stupor ; moans and raves without ceasing ; erysipelatous redness on the left nates and hip ; behind the trochanter, a gangrenous patch has appeared, from which the cuticle has separated, three grains of quinine every fourth hour ; blister to the nape of the neck ; wine and water. In the evening there was profuse perspiration, and the face became deeply flushed.

25th. No material change in the symptoms ; insomnia ; moaning, screaming, and raving ; countenance more sunk.

26th. Died at four o'clock this day.

Post mortem examination.—Patchy vascularity of the pia mater, substance of the brain firm ; a considerable quantity of reticulated lymph effused on the right lung, with corresponding hepatization. Numerous and very small abscesses scattered over both lungs, containing an imperfectly formed pus, and all situated either on or immediately contiguous to the surface. The bronchia contains a large quantity of frothy mucus. Numerous black patches, evidently congested blood, in the substance of the lungs. Hepatization in its first stage existed, corresponding to the deposition of lymph on the surface ; abdomen healthy.

On cutting into the right hip joint, the synovial membrane was found inflamed, a quantity of brownish red purulent matter in the joint. Through an ulcerated opening in the capsule, the matter had passed out, separating the muscles from each other, and detaching the periosteum from the upper third of the femur. In the left leg, the periosteum was separated from the entire of the tibia, inflamed, thickened, and reticulated ; the synovial membrane of the left ankle was also inflamed, and covered with a secretion similar to that in the right hip joint ; the epiphysis

was completely separated from the lower end of the tibia. In slitting up the left femoral vein, a fibrinous clot was found, and a small quantity of purulent matter issued from its cavity.

CASE 2.—Mary Murray, aged 13, was admitted July 16, 1831, into the Richmond Hospital, labouring under much fever; pulse 130, and small; pain and some swelling of the right knee joint, and upper part of the leg; no discoloration of the skin; unable to bear the slightest pressure; has been for some time exposed to great fatigue and hardship, to which she was previously unaccustomed; four days previous to her admission, was attacked suddenly with severe pain in the knee, which soon extended to the leg, and with frequent vomitings. In the course of the day, the tension and swelling increased so much, that the fascia was freely divided above the centre of the leg, a bolus of calomel and jalap, twelve leeches to the leg; stuping and poultice.

18th. Eight o'clock, A. M. Fever continues very high; raved during the night; the swelling has extended down to the ankle, which has become implicated; increased tension; obscure sense of deep fluctuation; a roseolar rash over the whole body; frequent bilious vomitings; insatiable thirst; bowels free; an incision was made down to the tibia a little below the knee, and on dividing the periosteum, a reddish coloured purulent matter was given exit to, in considerable quantity. Effervescing draughts, with tincture of opium, V. S. ad ξ viii.

19th. Delirium during the night; cessation of vomiting; bowels free; eruption nearly vanished; troublesome itching, and great heat of the surface; tongue loaded; excessive thirst; pulse 130; in the course of this day pain in the right side of the chest, increased by a full inspiration; leeches to the ankle; diaphoretic mixture; blister to the right side of the chest.

20th. Restless night; delirium; recurrence of the eruption, which has now assumed more the character of urticaria; swelling and tension of leg and ankle abated; abdominal ten-

derness ; hands and feet cold ; constant moaning ; pulse 160 ; leeches to the epigastrium ; calomel and opium.

21st. Slept two hours, awoke screaming ; great heat of the scalp ; eruption more extended ; patches more elevated ; pulse 150 ; sordes on the gums and teeth ; bowels confined ; leeches to the head ; castor oil draught.

22nd. Continues screaming, tossing of the head ; insomnia ; pulse very rapid ; sunken aspect ; heavy expression of the eyes ; dilated pupils ; involuntary stools.

23rd. Loss of vision, and increased dilatation of the pupils ; frequent screaming, laborious breathing ; mucous r  le ; slight convulsions ; consciousness perfect ; death in convulsions. No examination of the body permitted.

CASE 3.—Patrick Keegan, aged 13, admitted 21st of May, 1831, under the care of Dr. Hutton, with pain and swelling of the right arm, extending from the shoulder to the elbow, intolerance of the least motion or pressure ; slight œdema, and much constitutional disturbance ; tongue dry and brown, and its apex red ; eyes heavy, countenance sallow and anxious, severe headach ; nausea ; high coloured and scanty urine ; pulse rapid, and skin very hot ; had an attack of rigor yesterday, followed by hot and sweating stages. It appears that he had enjoyed good health till twelve days since, whence he was suddenly seized with severe pain in the right shoulder, which gradually extended all down the arm. At the end of a week swelling commenced, and the pain became more severe. Diaphoretic mixture with tincture of opium ; emollient enema ; leeches ; saline draughts, and, in the evening, calomel and antimonial powders every fourth hour.

23rd. An incision was made down to the bone at the upper part of the arm, and the periosteum which was found thickened, vascular, and easily separated from the bone, was freely divided. The eighth of a grain of opium added to each pill. In five days afterwards, the pain and swelling had nearly subsided, and he was free from fever, and eventually he recovered.

CASE 4.—Henry Sevan, aged 16, was admitted into the Richmond Surgical Hospital on September 30, 1831. A few days previous to admission, he fell from a height of about fifteen feet on his left side; on admission his countenance was remarkably indicative of severe suffering, he complained of violent pain in the *left hip joint*, tenderness on pressure all over the articulation, particularly anteriorly, the thigh was flexed, and the foot inverted and resting against the opposite limb. In my absence he was ordered a bolus of calomel and jalap. In the evening he complained of severe pain in the right ankle joint, and in a short time, the integuments covering it became inflamed. Symptoms of cerebral excitement now manifested themselves, he was almost constantly raving or screaming, but when spoken to, answered rationally.

October 1st. Raving continues, screaming more violently; skin very hot; pulse rapid and firmer; tongue much loaded with a cream-coloured mucus; insatiable thirst; increased redness and swelling of the ankle; less pain in the hip; cupping behind the trochanter, and twelve ounces blood removed; eighteen leeches to the right ankle; bolus repeated; head shaved and cold lotion applied.

Seven o'clock, evening.—No abatement of the symptoms, redness extending up the leg; constantly screaming; bowels well opened; two grains of calomel and one of Dover's powder every fourth hour.

2nd. No rest last night, incessant screaming; increased redness and swelling of the leg, an incision more than three inches long made down to the bone, and nearly four ounces of healthy pus given exit to, from beneath the periosteum. At twelve o'clock the screaming abated, but he became comatose, and died at nine o'clock in the evening.

Post mortem examination twelve hours after death.—A very vascular connexion between pericranium and bone was found, and a slightly augmented vascularity of the pia mater. The right ankle joint was filled with healthy pus, and from the

articulation upwards four inches of the periosteum were separated from the bone by pus; the bone was very vascular, the synovial membrane of the ankle joint was thickened and vascular, the cartilages were healthy.

In the left hip joint, there was a collection of purulent matter as in the ankle, and the synovial membrane presented the same diseased appearance.

CASE 5.—*Synovitis and Periostitis from injury.* George Walsh, aged 10, admitted October 15, 1832, into the Richmond Hospital. Eight days before his admission, he fell from a wall about eight feet high on his side and hip, and two days subsequently he was *suddenly* seized with severe pain in the right hip, which confined him to his bed. The pain also extended over the external fossa of the ilium, to the sacro-iliac symphysis. Shortly after admission, he was attacked with sudden and severe pain of the left joint, his countenance was of a sallow hue, and, in a remarkable degree, indicative of extreme suffering and anxiety; the tongue was covered with a brown fur in the centre, and a stripe of white on either side: the pulse small and very rapid; *excruciating* pain increased by the slightest pressure or the slightest motion, or even by allowing the limb to hang unsupported, it attacks him in paroxysms, causing him frequently to cry out. On the right side it occasionally shoots down to the knee. The right hip joint is swollen; the superficial veins are dilated and turgid, the fold of the nates is obliterated, and the patient lies partly on the left side and partly on his face, with his limbs flexed; excessive thirst, and his skin is dry and burning; V. S. ad $\frac{3}{4}$ viii.; twelve leeches; stuping and poultice; two grains of calomel, and two of James's powders every third hour.

16th. In the early part of the night, the paroxysms of pain were severe and frequent, but afterwards he slept four hours, frequently moaning; bowels free; the blood drawn from his arm yesterday shewed no buffy coat, the clot was large, but

firm and florid, the serum in very small proportion; calomel and James's powder repeated.

17th. Aggravation of all the symptoms; pulse 120, and feeble; countenance more depressed; purging and griping; the calomel and James's powder omitted; ordered saline diaphoretic mixture, with tincture of opium, a large blister over the ilium.

18th. Rested well last night; pain abated; greatly relieved; complains chiefly of weakness.

20th. Skin cool; tongue cleaning; pulse 96; diarrhoea; pain less severe; still frequently moans; pulv. ipecac. comp. gr. iv. ter in die.

22nd. Profuse ptyalism; mouth slightly ulcerated, better in every other respect; ordered to gargle with the solution of the chloride of soda, syrup, and water.

24th. Improving; mouth still very sore, and ordered to be brushed with a solution of nitrate of silver, consisting of ten grains to each ounce of distilled water.

30th. Considerable swelling and prominence of the nates of the affected side, severe pain on pressure, and at one point an obscure sense of deep fluctuation. An incision made down to the bone through the periosteum, near the sciatic notch; the membrane thickened, and easily separated from the ilium; no fluid evacuated at the time, but in two days afterwards a considerable quantity escaped. He ultimately recovered.

CASE 6.—Peter Neale, ætat. 12, admitted January 11, 1833, into the Richmond Surgical Hospital. Four days previous to admission, he fell from a wall of moderate height, on the left hip, which was so much contused, that he was unable to stand upon the limb, and was carried home. The pain and constitutional disturbance daily increased. On admission, it was found that the left hip joint was very tense and swollen; the pain was so excruciating, that he was unable to move in bed without assistance. His countenance anxious, sunken, and expressive of great and intense suffering; tongue greatly

furred; black sordes about the teeth; pulse rapid; some cough; raves at night; ordered leeches to the hip; two grains of calomel and half a grain of ipecacuanha every third hour.

12th. No improvement; constant delirium during the night; yet, when spoken to, he answers rationally; screams and cries loudly at times; leeches repeated; fomentations and poultices; head to be shaved and cold lotion to be applied; calomel and ipecacuhana continued.

13th. Screams without intermission; pain in the left hip unremitting; delirium; countenance more sunk; tongue white; thirst great; headach; feet cold; manner agitated; *pain* in the *right shoulder and elbow*, but no swelling; hip extremely tender and more tense, swollen, and elastic to the feel. An incision about two inches long made at the upper part of the hip, behind the anterior superior spine of the ilium, through the gluteus, the lips of the wound separated widely, so as to render the incision almost oval in form; relief expressed immediately, no fluid escaped, and a careful examination did not indicate the existence of any under the periosteum. Twelve grains of calomel and a grain of opium in nine pills, one every third hour; blister to the arm; stuping of the feet and legs.

14th. Mouth affected by the mercury; rested well during the night; respiration catching, and forty-two in the minute; pulse 150; considerable diminution of pain; increased power of motion in the limb; slight cough and wheezing respiration; great thirst; drowsiness, and tendency to coma; occasionally muttering; delirium and screaming; a peculiarly wild and frightened look; an anodyne draught with antimonial wine at bed time; a blister to the chest.

15th. During the night an increase of delirium, restlessness and loud screaming up to half-past seven o'clock this morning, when he died.

Post mortem examination five hours after death.—The muscles remarkably rigid; lividity of the lips; frothy mucus and sordes on the gums and teeth; large violet patches from

congestion, on the posterior part of the trunk. Pia mater and brain more vascular than usual ; a considerable quantity of high coloured serum effused into both pleuræ ; extensive, close, and recent adhesions between the pleuræ ; both lungs inflamed, and so highly congested, that portions of them sunk in water, numerous small abscesses on the surface, and some in the substance, containing an imperfectly formed pus, and having their cavities lined by a thin coating of lymph, and the surrounding pulmonary tissue very vascular ; intense bronchitis, particularly of the divisions of the left bronchus ; upwards of two ounces of high coloured fluid in the pericardium ; the serous covering of the heart of a milky white colour. On cutting through the left gluteal muscles, matter issued from numerous small points. The muscular fibres were of a deep red colour, the periosteum was detached from the entire of the ilium, by a quantity of a dark brown pus, which passed through the great sciatic notch, and separated the membrane from the whole concavity of the bone, which was of a pink colour. The fluid had passed, through a round ulcerated opening in the capsule of the hip joint, from the acetabular cavity. Small portions of lymph were found on the head of the bone, the synovial membrane covering the fatty mass at the bottom of the acetabulum, was inflamed and covered with lymph. There was no ulceration of the cartilages. The right shoulder joint healthy ; in the right elbow a fluid in moderate quantity was found, resembling that contained in the hip joint, and under the periosteum of the ilium.

(To be continued.)

BIBLIOGRAPHIC NOTICES.

Notices concerning Works on the Practice of Physic, Surgery, Pathology, and Physiology, recently published in Germany.

(Continued from page 239.)

THE next work I shall notice, has been very recently published in Hamburg, it is entitled :—

Ueber den Zustand der Heilkunde und ueber die Volkskrankheiten in der Europäischen und Asiatischen Türkei, ein Beitrag zur Kultur und Sittengeschichte. Von Friedrich Wilhelm Oppenheim, Doctor der Medecin, &c. &c. 1833, 8vo.

An Account of the State of Medicine in European and Asiatic Turkey, together with remarks on the most prevalent diseases, &c. &c.

This work relates to a country but imperfectly known by the more civilized nations of Europe, and rarely visited by medical men of great scientific attainments, such as Doctor Oppenheim. This gentleman, educated at Göttingen and Heidelberg, is well known in Germany by various medical essays, particularly an Account of the State of Medicine and Surgery in Italy, which originally appeared in the Journal of Foreign Medical Literature, published at Hamburg, by my friends Dr. Julius and Dr. Gerson, and from which I made copious extracts in the Edinburgh Medical and Surgical Journal, for January and April, 1826. Dr. Oppenheim visited all the medical schools of France and Great Britain, and in Hamburg had distinguished himself as a skilful operator and excellent physician, when, stimulated by a laudable desire to investigate the eastern parts of Europe, he left his native city, at a time his private practice was daily on the increase, and joined the Rus-

sian army at the commencement of the Turkish campaign. His surgical ability, and the high testimonials he brought, soon raised him to a situation of trust and rank in the medical staff, attached to the Russian army; and he proved himself worthy of confidence not only in the field, but during the devastations committed at Adrianople and Varna, by the plague. The account of the nature, progress, and symptoms of the plague, which our author is shortly about to publish, will, no doubt, add a vast fund of information to our stock of knowledge concerning this dreaded epidemic. The opportunities Dr. O. enjoyed of making himself master of the important subjects he wished to investigate, were such as have, perhaps, never fallen to the lot of an enlightened Christian physician, for at the request of the Turkish authorities, he obtained permission from the Russians to enter the service of the Sultan, and henceforward attended the army of the latter in its various expeditions in Europe and Asia, during a period of nearly three years, and was honoured not merely with the acquaintance, but the friendship of some of the most eminent commanders and statesmen of Turkey, and became so great a favourite, as to live on terms of intimacy with several Turkish families, whose high respect for his attainments induced them to waive their objections to him as a Christian. With such a talent for observation, and such opportunities for observing, it is not a matter of surprise, that the results of Dr. O.'s labours have furnished a much more accurate view of the state of medicine and surgery in Turkey, both European and Asiatic, than any before published. It is proper to remark, that Dr. O.'s observations apply only to the provinces and not to the capital; the state of general knowledge and of medicine, is, of course, very different in Constantinople, where the presence of the court has collected and concentrated the few straggling beams of light, which here and there shoot through the intellectual darkness that broods over the vast dominions of the Sultan. Of the capital Dr. O. says nothing, but refers us to the account given in Hufeland's Journal, last year, by Prince Demetrius Maurocordato, whose description of the state of medicine in Constantinople, Dr. O. praises as very accurate and satisfactory.

The father of medicine in Turkey was an Arabian, named Lochmann, appointed in the seventeenth century by Mahomed, to discharge the sacred functions of physician. The miracles performed by Lochmann were numerous, and tradition has recorded them in glowing colours: he was a wandering dervise, and taught his art to the brethren of his order, who, retaining to this day the precious secrets he revealed,

continue by birth-right the practitioners of Turkey *. As might be expected, this religious order of physicians, are greater proficient in superstition than in practical medicine, and except being acquainted with the virtues of a few plants, they absolutely know nothing. It is true, indeed, that they attempt to acquire confidence by appealing to supernatural agency, divination, astrology, talismans, and cabalistic figures.

Sometimes they attribute the origin of disease to the special wrath of God, in others to the interference of devils, but never perform the ceremony of deprecation or exorcism, without a multiplicity of rites and sufficient pay. Where money is given in the expected quantity, their prayers are endless, their beads are told *ad infinitum*, picked sentences of the Koran are sewn together and given to the patient to swallow; or when a fluid menstruum is preferred, the holy words are written with chalk upon a piece of board, this is washed, and the water with which the ablution is performed, forms a draught potent in proportion to its impurity. Amulets, however, form the favourite charm of the Turks, and over the whole of the east, Mahammedans,† Jews, and Christians, appeal to their protection, when threatened or overtaken by misfortune. Hence, few die without wearing two or three amulets, to whose safe guardianship they had intrusted their lives. They generally consist of a scrap of paper, containing a sentence from the Koran or Bible, embellished with cabalistic figures, and folded in a triangular shape, enclosed carefully in a little bag, and worn next the skin, either by means of a string hanging from the neck, or by being stitched inside the turban. Some amulets, supposed to possess a spell capable of protecting from ball and dagger, are sold at an enormous price. Thus, says Dr. Oppenheim:—

“ After the defeat and death of Wihli-Beg in Monastir, an amulet (Nusko) was found on his body, which he had purchased for sixty thousand piastres. The Selictar (sword bearer) of the grand Vizier, had its virtues renewed by a dervise, and then wore it himself; I asked him how it happened, that the fate of its late possessor had not rendered him sceptical concerning its protective powers. He answered, that nought, save the holy will of the Sultan, exceeded this Nusko in power, and that so long as he who wears it refrains from provoking the displeasure of his sovereign, he is secure against the hottest fire of the enemy or the poignard of the assassin.”

The unsuccessful Turkish suitor, invokes his amulet to

* The Turks, with a happy knack of distorting Frankish names, have confounded Hoffman with Lochmann. Thus, Hoffman's liquor they call Lochmann-Rouch.

† The name of the prophet is pronounced *Mahammed*.

soften the obdurate heart of his mistress, and those who are afflicted with ophthalmia, ague, and various other diseases, often place their whole reliance upon the virtues of a scrap of consecrated paper. As the dervises who practise the healing art, can alone infuse power into these amulets, they foster the public credulity, and by selling them at an enormous price, contrive to lose nothing by the confidence of their patients being transferred from themselves to the amulets they manufacture. This is silly and melancholy enough; but after all, while the newspapers of Great Britain advertise every day hundreds of specifics; while there are purchasers in abundance for quack medicines, such as Morrison's pills, which heal every disease; while the aristocracy of the country besiege the door of St. John Long; when a nobleman and a member of parliament, still considered sane by his constituents, has sworn in a court of justice, that St. John Long's frictions caused globules of quicksilver to exude from his skull; when a barrister of reputation in Dublin believes and asserts that the same liniment drew a pint of water from his own brain; when half the community of Dublin believed the miracles of Hohenlohe; when a commission, appointed by a grave and learned society of physicians in Paris, has reported favourably of the miraculous effects of animal magnetism; when we recollect all this, I say, ought we to indulge too freely in ridiculing the Mahammedans for their trust in amulets, or the Turkish matrons for their dread of the evil eye of the stranger, and their belief that all the maladies of their offspring spring from its blasting influence? Another superstition of the Turks is, an observance of lucky and unlucky days for prescribing or taking medicine, and it is singular enough, that they consider Friday, the most unlucky day of the week with ignorant Christians, as the most propitious, while Tuesday is regarded as peculiarly unlucky, and no one thinks either of the exhibition of drugs or the performance of operations, even in the most urgent cases, upon a Tuesday. It was on a Friday that the memorable flight of Mahammed took place, by which his life was saved. Every one in society, who can afford to pay for such useful information, takes care to purchase from the astrologers an interpretation of his destinies, as fixed by the stars that presided over his nativity, and each person has his own lucky and unlucky day of the week. It is well known, that even the mighty genius of Napoleon was enslaved by somewhat a similar superstition. The total ignorance and incompetence of the native practitioners have not altogether escaped the observation of their countrymen, for it has been long ago remarked, that a foreign physician, particularly if a Frank, is supposed by the Turks in general, to be possessed of

far superior knowledge, and accordingly they are followed with avidity. Whoever appears in any part of Turkey dressed like a Frenchman, an Englishman, or a German, in fact, whoever wears a hat and not a turban, is immediately looked on as the possessor of medical knowledge, and is at once called "Hekim Baschi," and must *nolens volens*, immediately enter upon practice, for the Turks crowd round him, and hold out their hands that he may feel their pulse, which, in their opinion, is all that is necessary to enable the physician to form a correct diagnosis, and they believe, therefore, that when the pulse has been felt, nothing more is required to give an insight into the nature of their disease, and the proper method of treatment. Others of the crowd, thinking themselves sufficiently acquainted with the nature of their own maladies, seek in the physician only a person to supply them with the remedies they themselves indicate, and accordingly, one applies to him for a vomit, another for a purgative, a third for a medicine to produce wind, another for one to expel it; for the ancient pathology, that diseases are caused by an excess or deficiency of wind in the various organs and cavities of the body, is still common; thus, a headach is caused by wind in the head, dyspnœa by wind in the chest. The physiology of respiration is thus rendered very simple, and the trachea becomes the air pipe not merely of the lungs, but of the whole body.

The encouragement thus given to foreign practitioners, has generated the greatest abuses, for as there are no means of ascertaining the acquirements of strangers, many, induced by sordid views, embark on a system of barefaced quackery, and thus persons who have followed other employments at home, are suddenly physicians in Turkey. Dr. Oppenheim was invited to attend a consultation with an eminent French physician at Smyrna, who candidly told him, that the only preparation he had for the profession was, service in the army as drum major! Among the staff surgeons of the Turkish army, was a Maltese, who had been a letter-carrier at Corfu, and an Italian captain of a merchant vessel, who had been shipwrecked on the coast of Asia Minor. A Genoese gentleman, implicated in the late revolutionary attempts in Piedmont, and who had long served in the army, applied to Dr. Oppenheim, who gave him sixteen recipes, by means of which he was set up in the world, being soon afterwards appointed physician to the governor of Jambul! Nothing can exceed the heterogeneous materials of which the mass of practitioners is composed; foreigners from all countries, and of all trades, but chiefly Greeks, Jews, and Armenians, the religious orders of all the different forms of worship that are professed in Turkey, besides gipsies, barbers, and

old women. Of the foreigners some are well educated, and a few, whose names Dr. O. mentions, are excellent surgeons and experienced physicians, but such are "few and far between." It is a pity, that the state of medicine is so low in a country, where the inhabitants esteem so highly the medical art, and where all are inclined to respect physicians; by the Turks, a skilful physician is almost ranked as a saint, and the appellation "Hekim," is the surest protection against either religious or political persecution. In the last campaign against the Russians, often, says Dr. O., was the uplifted sword of the half barbarous Turk arrested on the cry of "Hekim" being uttered by his vanquished foe. The modern Greeks give the title of Excellency to the physician, and old Homer estimated the value of a good surgeon and physician very precisely, in saying, that he was worth half a dozen colonels!* It may be here mentioned, as a curious fact, that the formation of the immense empire of Great Britain in the East Indies, was, in its infancy, greatly aided by the respect entertained for the acquirements of an English physician named Boughton, the successful exertion of whose medical skill enabled him to obtain from the native princes, what the East India Company had for forty years in vain struggled to possess, the liberty to make a permanent settlement and build a factory. There is a particular district of Greece called Sagor, in the Paschalick of Janina, where the profession of medicine is, as it were, the national characteristic and the chief occupation of the inhabitants, whose right to practise is hereditary, and whose knowledge consists in recipes and rules of treatment, handed down from generation to generation. Three or four villages in this district are complete medical hives, sending forth their annual swarms of physicians, who spread themselves over the whole of Macedonia, Albania, and Rumelia, and, in short, over the whole Turkish empire. They follow the good old Greek fashion, which sanctioned this lazy sort of hereditary diploma, and looked on the descendants of Esculapius as accomplished physicians from their very birth. In other states, it is not rare to find a predilection for certain trades and manual occupations, which are cultivated almost exclusively by the inhabitants of certain districts, who migrate in multitudes over the whole of Europe in search of employment. Thus, Bavaria supplies broom girls, Savoy organ players and bear dancers, Lombardy her workers in plaiter of Paris and makers of images, to all neighbouring and even

* It is difficult to assign their proper rank to many of the chiefs and minor heroes of the Iliad. In calling them colonels, I mean no offence to the dead.

many distant countries, while in France, every shoe black is a native of Auvergne, every gate porter is from Switzerland, and in Spain, every water carrier comes from Galicia; formerly Ireland supplied London with sedan chairmen, and now with coal heavers.* It was reserved, however, for *Sagor* to stand forth as the productive mother of doctors, an offspring scarcely less dangerous than that which the soil of Bœotia yielded, when the crop of armed men sprung up before the astonished eyes of Cadmus.

Jewish physicians abound in Turkey, and are not a whit better informed than the Albanians. They wander about the country, with their apothecary's shop upon their back, and are, in fact, perfect medical pedlars. Their traffic is not confined to the sale of medicines alone, for they vend cosmetics of all sorts, soaps, oil of roses, charms, and colours. The poorest of this class carry wallets, and walk the streets and bazaars, at every pace uttering the shrill cry "ei Hekim!" "ei Hekim!" (*a physician, a physician.*) Now and then you may see them stopped in the street by some unhealthy looking Turk, whose pulse they feel, and instantly roar out, "bilirim senin hastalik," (*I know thy disease.*) and without asking the patient a single question, they open their wallets, give him a pill or a powder, which he swallows on the spot, after bestowing on the physician two or three half farthings (*paras*) for his advice and medicine! Knowledge came from the East, it has travelled slowly to be sure, but here it has arrived at last, and lo, our fees, formerly paid in gold, are *changed* into silver, and undergoing the rapid process of depreciation, the distant tinkling of brass may be heard even now by the ear, practised in the sounds of coming events! † As long as the fates permit, let the profession struggle against the adoption of this oriental custom, let it in this instance, prefer the usages of the West to the wisdom of the East, let it not be said of us, that we are "avari, ambitiosi quos oriens non occidens satiaverit." ‡ Strange as may appear the Turkish physicians, they are almost exceeded in singularity by their patients, who require the most extraordinary qualifications on the part of their medical attendants. Thus, nothing so enhances in their eyes the value of a physician, as his being able to tell every thing after feeling the pulse. By the pulse alone, he must know not merely the na-

* In the reign of Charles the Second of England, the number of Scotchmen who carried on the trade of *pedlars* in Poland, amounted to 25,000!—Vide article *Pedlar*, *Encyclopædia Britannica*.

† Coming events have shadows, why not sounds?

‡ Tacitus—*Agricola vita*.

ture of the disease, but must be able to say whether the patient slept well the night before, what he ate during the day, whether the bowels are open, &c. &c. After having once felt the pulse, the physician must put no question to his patient, for it is considered as a sign of ignorance; at his very first visit, he must declare, from the pulse, at what precise time the patient will die or recover. The governor of Adrianople, Halisch Pascha, once visited the tent of the Russian General, Paulin, where Dr. Oppenheim and two other physicians were attending at levee. Each of the three successively was presented to the Pascha, who made them feel his pulse; and when the ceremony was over, he immediately declared, that one of them was incomparably a better physician than the others, for said this wise Pascha, he felt my pulse much better!

"Often," says Dr. Oppenheim, "on presenting my passport to a Turkish officer, the moment he read the words '*Hekim-Baschi*,' has he turned out the guard and drawn them up, in order that I might feel the pulse of each. This, of course, I used to do with vast gravity and apparent attention, and the men were quite pleased upon being informed that they were in excellent health!"

Many of the knavish Greek physicians pay the domestics to give them private intelligence, concerning the diet, evacuations, &c. of their patients, whom they afterwards impose on by making them believe, that their sole source of information was the state of the pulse. When the physician, by means of the pulse, has declared the precise nature of the disease, and the exact moment of its termination, the Musselman requires him to give a certain medicine, to have some particular effect in determining some evacuation, which is to prove critical. No medicine gets the least credit, or in their eyes can be the least effectual, unless it produce sweat, urine, or purging. The Turk is fond of large doses, too, in order to produce more decided crises, and he always prefers medicine in the shape of a draught, or rather drink, (sherbet.) He dislikes emetics, and nothing will induce him to allow the exhibition of an enema. It is quite vain to endeavour to make him alter his diet, of this he cannot conceive the use. In the month of May, it is not unusual for them to submit to what is termed the spring cure. An active purgative is first taken, and afterwards the expressed juice of various plants, such as *Taraxacum*, various grasses, &c. are taken daily, along with a drink of whey. The most favourite purifier of the blood, however, is *viper broth*. The most esteemed vipers are caught in the neighbourhood of Adrianople, and are sent thence in great numbers

to Constantinople, and other parts of the empire. They are kept in wooden vessels, and when wanted for use they are drawn out through the bung hole. It is needless to remark, that this operation requires much caution and skill, in spite of which, as happened in an instance which Dr. O. himself witnessed, the poor apothecary is sometimes bitten. The bite often, but not always, proves troublesome, or even fatal. When this dangerous article of the *materia medica* has been safely extracted from the vessel, his head and skin are instantly taken off, and the animal is cut into thin slices, which are boiled with water to make broth. The most effectual of the means employed either for the prevention or cure of diseases by the orientals, is the bath (*Hamam*.) The long continued frictions employed, the stretching, drawing, kneading of the limbs and flesh, the pulling and working of the joints, &c., all tend to exercise a healthful influence; it is astonishing, what a command over the joints an experienced attendant at the baths possesses. He twists them in every direction, and you almost feel, as if he had performed on you a number of successive dislocations and reductions, following each other with surprising rapidity. In chronic diseases of the skin, gout and rheumatism, these baths are invaluable.

The public baths are very handsome, capacious buildings, of which there are several in each town. The bather undresses in a large and spacious hall, provided with benches, and having a fountain playing in the centre. He ties a silken girdle round his loins, and puts on a pair of wooden sandals, and is then introduced into the first chamber; which like the rest is lighted from above, and is flagged with marble. Its heat is moderate, and is intended to prepare the bather for the higher temperature ($99^{\circ}\frac{1}{2}$) of the second chamber, which is arched, and has the flags all heated from below. In the centre of the second chamber, is an extensive platform of marble, elevated about a foot above the floor, on which you stretch yourself at full length, while the attendant goes through the various manipulations on your body already spoken of. This finished, you proceed to one of the numerous alcoves or recesses with which this chamber is provided, and here the process of bathing, properly so called, begins; warm water flows from a pipe into a marble basin, the bather sits down naked on the warm floor, and his attendant, with a piece of cloth made of camel's or horse's hair, which he dips frequently into the water, forms a lather of a sweet scented soap, and with this rubs every part of the body, and finally, pouring warm water over the bather completes his purification. He is then covered with warm cotton cloths, and conducted into the outer hall, when he lies down for half an

hour on a bench, takes a cup of coffee or a glass of sherbet, and then dresses himself.

The expense of such a bath is so trifling, that it is in the power of even the poorest Turks to make use of them. Every where the baths for the different sexes are in different parts of the town. To the women they afford not merely the luxury of bathing, but the opportunity of meeting their friends and acquaintances. They have been described by Lady Wortley Montague, in colours more glowing than might appear seemly in the pages of a scientific Journal, and therefore, it may be prudent to omit the subject altogether, merely observing, that, as is natural, they are the chief strong holds of gossip and scandal, and afford the anxious mothers ample opportunities not merely of shewing their daughters to other matrons, but of seeking wives for their sons. In Turkey, the practice of letting blood in spring, formerly common in Great Britain, is still prevalent.

With regard to the manner in which the more respectable part of the medical profession is paid, it evidently evinces a great want of confidence, or rather extreme distrust. In England, it is commonly believed, that the word of a Turkish gentleman or nobleman, once given, may be implicitly relied on; but it is too clear, from the narrative of Dr. Oppenheim, that a most lamentable want of principle prevails even amongst the upper ranks. Wo to such a nation, for mutual distrust among individuals prevents all unity and energy of action on the part of the rulers; private corruption inevitably portends the public downfall.

“There is,” as Burke beautifully remarks, “a confidence necessary to human intercourse, and without which men are often more injured by their own suspicions, than they could be by the perfidy of others.”

The sick Turk, says Dr. Oppenheim, makes promises, the convalescent Turk breaks them. In consequence of this disposition, the physician is often obliged to draw up a specific contract in writing, and according to a legal form, before he undertakes the treatment of a case or the performance of an operation. The contract is deposited in the hands of a magistrate, who can enforce payment, and whose zeal in the discharge of this duty is quickened by the legal fee of ten per cent., to be deducted from the stipulated sum. It is not very rare, however, for the patient to evade the ends of justice, by paying the magistrate twenty per cent.; when this is done, the physician's contract too often turns out to be waste paper. These contracts, however, in general afford the physician tolerable security, and are especially necessary when capital operations are performed,

as without them he may lose not merely his fee, but his life, in case his patient dies, for the Turk considers the knife of the surgeon in the light of a weapon wielded by an enemy, and thinks himself called on to avenge the death of a relative after an operation. This is hard enough upon the poor surgeon, who, to avoid more fatal consequences, is often obliged to pay blood-money to appease the wrath of relatives. To avoid these consequences, the surgeon and one of the nearest relatives of the patient repair together to the *cadi*, if it be a small, or to the *mufti* if a large town, and obtain from him a *protection* (*fetwa*), by which the surgeon is secured against all persecution if the patient dies. Dr. Oppenheim, himself, felt the force of this Turkish antipathy to the performers of unsuccessful operations. After the battle of Monastir, on the 24th August, 1830, he amputated the leg of a wounded Deli:* the Deli died. In a few months, Dr. Oppenheim was sent by the Grand Vizier to inspect recruits at Pristina, and was invited to the house of the *Cadi*.

“ After the customary compliments, he asked me, ‘ Are you physician to the Grand Vizier? Did you operate on the Deli, Soliman-Aga?’ I answered in the affirmative. ‘ Then,’ said the Cadi, ‘ you behold here the father of Soliman-Aga, who claims blood-money from you, which money it is most just you should pay him.’ ”

Dr. Oppenheim being sufficiently acquainted with the usages and manners of the Turks, and depending upon the protection of the Vizier, was no way intimidated, and soon brought both the Cadi and Aga’s father to reason, by means of a few wholesome threats.

When a physician has treated a patient who dies of internal disease, he incurs no risk, unless the deceased held some important and lucrative government post; in such cases, the relatives and dependants of the deceased, being deprived by his death of their station and emoluments, are apt to wreak their vengeance on the physician, who, however, generally takes care to be out of the way on such occasions. At other times, medical men are employed to give opinions, concerning not the living but the dead! This may appear strange, but it is the fact, and it is for such opinions that they are sure to be best paid, for they have it in their power to make what conditions they please with their employers. In Turkey, whenever a go-

* The Delis form the flower of the Turkish cavalry, and their name means *madman*. They are so called from their frantic impetuosity in battle.

vernor of a province, or mufti, or any other *employé* of the government dies, the whole of the treasure in his possession immediately finds its way into the coffers of the state, therefore, it becomes an object of paramount importance for the family, to conceal, if possible, the death of their relative, until they have either made off with his money, or what is a safer method of proceeding, until they have used one portion of it to bribe the members of the divan into conniving at their keeping the remainder. The father of the present Pascha of Uskup, it is now ascertained, was buried four years before his death was announced. During the interval, his son had carried on all the public business in the father's name, and the signature of the latter was affixed to all official documents. During this period, medical advice was sought for in all quarters, and eminent physicians were even brought from Constantinople. They were consulted, but for very evident reasons, were never permitted to see their patient, a matter esteemed of little consequence in Turkey, provided the state of the pulse is accurately described.

"I must confess," says Dr. Oppenheim, "that being at the time but little acquainted with Turkish manners, I was any thing but pleased upon being sent for by Abduraman, the Pascha of Kalkandehl, to treat some patients in his harem. I was received by the Pascha with all those marks of distinction, which the Turk of consequence bestows on a Christian physician, when he has occasion for his services. After he had complimented to excess myself individually, and had extolled the wisdom of the Franks generally, he informed me, that his whole harem was sick, but that with my aid, he had little doubt that his three wives would be speedily cured. The first lady I visited was about twenty-four years of age, who laboured under catarrhal fever. I promised to cure her in a few days. The second was nearly twenty years old, and of a well marked strumous diathesis. She laboured under a chronic ophthalmia and herpetic eruption. My prognosis was, in her case, more cautious, but favourable; but I specified no fixed period for her recovery. In the third apartment, lay a lady about thirty years old, who had anasarca and ascites, and was also in the last month of pregnancy; her breathing was so much affected, that I feared also the existence of hydrothorax. As I afterwards learned, three months previously she had used the strongest medicines to produce abortion, but in vain. In addition, she had for the last year been afflicted with a badly treated ague; these circumstances led me to suspect organic disease of some of the abdominal viscera; I say suspect, for no examination of the abdomen would be permitted. I told the Pascha, that she would be probably delivered of a still-born child, and that she would not survive its birth many days. *Bakkaalom! Allah Kaerim! Insch Allah!* (we shall see; God is great—God be merciful) exclaimed

he, and inexperienced as I was, I little dreamed that these were mere stereotype* expressions. The Pascha appeared to take the liveliest interest in this lady's state, and required me to feel her pulse four times a day, and to send him, as often, a report concerning her health. Whenever I spoke to him on the subject, his uniform reply was, 'Give her, I beg you, the best medicines you have; right strong medicines, and she will yet recover—God is great!' The dreaded day came at last; she was delivered of a dead child, and in two hours the harem resounded with the cries of the female slaves. In the east, the females are the first to announce either joy or sorrow. If any thing happy occurs, they utter a cry of joy, modulated by a rapid and quivering motion of the tongue against the palate and teeth. When sorrow is to be expressed, the cry is longer and sharper; the shrieks of the slaves in question were decorously loud and protracted, and they rent their garments and tore their hair. I sought not to be the first bearer of the news to the Pascha, whose anger I dreaded; when I arrived at his apartment, I found that he had already learned the sad news, and I felt greatly astonished at finding the man, who had been all anxiety and alarm at my former visits, now quite composed and tranquil. When I entered, he exclaimed '*Allah Kaerim.*'"

In the course of a short time, all the courtiers and principal officers had come in successively, each, as he entered, using the same invariable phrase addressed to the Pascha, "she is dead, thou shalt live."

The mother and child were consigned to the grave before evening, for in Turkey, among the great, there is no lying in state, among the poor no waking. The believer in the Koran hastens to inter the body of his relative, with as little delay as possible, for every moment that the body after death remains above ground, is spent by the soul in agony. The corpse is wrapped in a cerecloth, and committed, without a coffin, to the grave. The grave is about two feet deep, and is covered over with boards, on which the earth is heaped; the head of the body is turned towards Mecca. This practice of burying so quickly, must, in many cases, occasion persons to be buried alive, for it is followed by the whole population of the country, Jews and Christians, as well as Mahammedans. The day of his chief wife's death was marked by no unusual occurrence in the house of the Pascha. The inmates conversed, followed their occupations, and eat their meals just as if nothing happened: one alteration was indeed observable; during the lady's

* In the original "*diese stereotypen ausrufungen*," stereotype exclamations—a strong and original expression of Dr. Oppenheim.

illness, every one had spoken of her state, and evinced the greatest sympathy for her sufferings, now, not a syllable was uttered about her. It was the same in the harem, where Dr. Oppenheim, who had now learned the use of the expression, "she is dead, you shall live," found the other two wives of the Pascha very well pleased at what had happened, for they said that their departed friend, who, as the eldest, held the reins of authority in the household, had not led them a very comfortable life of it. These ladies made Dr. Oppenheim a present of garments embroidered in the harem, and the Pascha, well contented with his services, sent a guard of honour to accompany him two days' journey from his residence.

Apothecaries there are none in Turkey, and no shops for the sale of medicine, except at Constantinople, and one or two other large towns. Indeed in a country, where the physician is seldom able to write, such shops would be useless. Every physician, consequently, mixes the medicine for his own patients, and is surrounded in his office by a chaotic confusion of gallipots, pill boxes, drugs, &c. The labels are most curious, and present a truly polyglot assortment of Greek, Latin, Hebrew, Italian, &c. The correctness of the orthography and grammar of these labels, may be judged of by one specimen, "*unguenti diversi*," which adorned a box in one of the best shops in Adrianople. Good medicines are to be had at Smyrna, Salonica, and Constantinople, to which places they are imported from Marseilles, Trieste, and Venice; as they find their way into the interior, they are more liable to be adulterated. Where there exists nothing like a medical police, no check upon such mal-practices, it may be readily conceived, that no restraints are placed on the sale of poisons, and, consequently, poisoning by design and poisoning by accident are very frequent. Indeed, it occasionally happens, that a patient coming to a doctor, gets medicine weighed in a scale still soiled with corrosive sublimate or arsenic, and in quite sufficient quantity to despatch the unfortunate sufferer. Often too, it happens, that inexperienced beginners and ignorant pretenders, give powerful medicines in poisonous doses, in which case the writhings of the patients are interpreted as symptoms of their being possessed, and forthwith the Turkish dervise and the Christian Priest are in requisition, and proceed simultaneously with their different forms of exorcism. The precaution of having recourse to the rites of two different religions, is taken to avoid the possibility of mistake or failure, for say they, we cannot *a priori* tell, whether our friend is possessed by a Mahammedan or by a Christian devil.

Poisoning by design is still more frequent than poisoning

by accident, and our good honest Irish practitioner, who regards the oath of Hippocrates to be quite as unnecessary as the oath he is obliged to take against the Pretender, will understand better the necessity of many of the clauses ascribed to the father of physic, when he is told, that in the east, the physician is too frequently, to this day, the venal instrument of such heinous mal-practices. Indeed, according to the religious views of many Turks, it is no sin to poison an enemy, for the attempt to do so will assuredly fail, if he is not fated so to perish ! Besides, it is merely a measure of self defence, for if you do not anticipate your enemy, he is sure to poison you.

“ Melancholy, as it is,” says Dr. Oppenheim, “ to witness such mischievous misinterpretation of a Mohammedan dogma, it is still more melancholy to see persons who profess Christianity engaged in the same guilty course, for it cannot be denied that too many of the native Christians of the Greek Church are willing agents upon such occasions. In truth, no honest person ought to engage himself as domestic Physician to any great man in Turkey, for if he be called on to poison, and refuses, it may cost him his life. Of this I myself had a convincing proof. The late campaign of the Turks against the Albanians was brought to a successful conclusion, not by superior courage, numbers, or discipline, but by craft and treachery. Two of the most powerful foes of the Sultan, Whely-bey and Asslan-bey, surnamed the Lion Chief, were invited, during a truce, to witness a review of the Turkish regular troops, which to them was a matter of great interest and novelty. The Vizier had it so arranged that they were both shot dead as they were passing in front of one of the battallions. The Vizier’s son, Emin, Pascha of Janina, ensnared and despatched some of his most formidable opponents in a nearly similar manner at Janina. One evening at levee, the Grand Vizier made a sign for me to remain, and when all the courtiers had left the room, he ordered in coffee, pipes, and a chess board, and I then found myself alone in company with a man who expected and received unconditional obedience from every one of his attendants, and at whose nod more than one hundred thousand heads had fallen. Having signified that I should be seated on the divan, he smoked, but according to etiquette, I left my pipe untouched ; and when we had made a few moves at chess, he raised his head, looked fixedly into my eyes, and said, ‘ Hekim-Baschi, I have enemies, you can and will assist me !’ He then made the sign for me to retire, which of course precluded the possibility of my replying. I made my obeisance, and rode home greatly agitated and alarmed, for the meaning of the Vizier’s words was but too intelligible. At that time I was attending two Albanian Chiefs of note, who were afraid to trust themselves to the care of the Vizier’s physician, and who had applied to me as an officer of the staff for advice. The Vizier was aware of this, and wished me to despatch my two patients. I revolved in my mind the difficulties of

my situation, and saw no other method of escaping than by making large pecuniary sacrifices, in the way of bribe, to the Vizier's avaricious *Seraff*, (Pay-master,) and his *Grammatiko*, (Secretary.) In the mean time I feigned sickness, and remained at home. Twelve days had elapsed since my interview with the Vizier, and nothing remarkable had occurred. On the morning of the thirteenth day, my servant brought in my pipe and coffee as usual; I had nearly finished the cup, when I perceived an unpleasant taste, which excited my suspicion; I immediately took an emetic, and hurrying to the apothecary of the forces, he immediately recognized in the cup nearly two drachms of corrosive sublimate, upon which I swallowed the whites of several eggs, and experienced no further bad effects. Though the favour I enjoyed at court, and the prominent station to which I had been advanced in the medical department of the army, had made me an object of envy to many, each of whom might wish to see me removed, yet it was but too evident, that the blow aimed at my life had descended from a higher quarter, and, accordingly, I used every exertion to obtain a passport (*buerouldi*,) and, at last succeeding, hastily quitted Turkey."

Such attempts as that made on the life of Dr. Oppenheim are very frequent in Turkey, and are too often successful. Hence, it is usual, when speaking of any one who has become remarkable for power, influence, or wealth, to observe, "*He will probably soon die of poison!*" Hence, also, the avidity with which the rich cultivate the friendship of every newly arrived physician, particularly of a Frank. They are anxious to purchase his services, in order that he may not be employed by others to poison them. Of course, where poisoning is so frequent an occurrence, the feelings of a Turk of rank are by no means enviable, particularly when he is sick. It is then that he suffers mortal fear of being poisoned, and to prevent such a disaster, he always takes the precaution of making either the physician or a slave take part of the medicine by way of trial. The illness of the master thus sometimes undermines the constitution of the slave, who is found in this extraordinary service to undergo a long-continued series of vomitings and purgations. Of course, they at least must offer up sincere prayers for his recovery. When a bottle of physic is opened, and the dose measured out, it is again immediately sealed up with the master's private seal, to prevent the introduction of any poison. It is for this reason also, that the Turks are so fond of getting medicine from the hand of the physician who has made it up, for they thus render him responsible for its effects. In this country, such a precaution would perhaps only render a patient more liable to be poisoned. Our author next gives us the particulars of a visit to another harem, which are so cha-

racteristic of Turkish manners, that I cannot refrain from giving the details in the Doctor's own words :—

“ Like every body else, I felt a strong curiosity to get a peep at the beautiful females annually imported in such numbers from Georgia and Circassia to Constantinople, where they are brought at a very early age to be sold and distributed all over the empire, to serve their masters as servants or as mistresses. I was also extremely anxious to witness the domestic arrangements of these little female colonies: fortune was propitious, and soon afforded me the desired opportunity. The favourite wife of Kiaja Bey, an officer high in the confidence of the governor of Adrianople, fell sick. The Pascha, who had great confidence in me, recommended my services, which were accepted, and a black eunuch was sent to my quarters to accompany me to the harem. It lay about an English mile from the residence of Kiaja Bey; we first knocked at a small wicket, which was opened, and on entering, we found ourselves in a garden, tastefully ornamented, and containing a light and airy summer house, near which the cooling waters of a fountain played into a beautiful basin of white marble. I was directed to seat myself near the fountain, and was immediately served with a pipe and coffee, while preparations were made in the harem for my reception. In quarter of an hour, I was conducted through the garden to another door, which was opened by a female covered with a veil, who it seems was the guardian and turnkey* of the harem. I was now led through another garden to the building of the women, which was evidently very populous, and I could distinguish the curious faces of children and slaves, white and black, peeping at me in every direction. At last the door of the sick lady's room was opened, and I entered into a very handsome but small apartment, with closed blinds and hung in red. The patient lay on pillows, placed on the carpet near the divan, and was so entirely covered from head to toe with white cloth, that I could only guess that she was present. I seated myself on the divan, close to her head, and now all unnecessary attendants were ordered to withdraw, so that I was left in company with my patient and interpreter,† the matron already spoken of, and two little children of the sick lady. All the questions I asked were answered from under the cloth, with simplicity and clearness, and many of them, which, in some of our young ladies might have excited *mauvaise honte*, were replied to in the most natural and easy manner. On my desiring to feel her pulse, one small white hand and then another made its appearance from under the cloth, and when I asked to see her tongue, she raised the cloth, so as to disclose the face of a beautiful brunette, about twenty years of age. This last

* This is not exactly the word used by Doctor Oppenheim—it seems, however, appropriate.

† Probably the black Eunuch.

act, apparently, was an effort which shocked the prejudices of my fair patient, for immediately, like a snail that suddenly withdraws itself into its shell, she shrunk back under the cloth, and I then quitted the apartment, and having put some necessary questions to the matron, I was brought into the *selamlick* or the *boudoir* of the master of the house, where I was again regaled with a pipe and coffee.

“Quite pleased with this visit, I was brought into the presence of *Kiaja Bey*, who, on being informed that if my patient followed the prescribed directions, she would be well in a few days, ordered me to be honoured with a pipe and coffee, and a purse of five hundred piastres. My prognosis was confirmed, and the recovery of the lady contributed greatly to raise me in the estimation of the Pascha.”

ROBERT J. GRAVES.

(*To be continued.*)

Principles of Geology. By CHARLES LYELL. Vol. III.

IN our third number we have given an analytical review of the two first volumes of this great work, which, in its perfect state, stands now before the public a monument of industry in the collection of facts, of penetration in the discernment of their bearing on the great phenomena of geological science, and of refined and subtle logic in the philosophic generalizations of which they are made the ground work. We have there endeavoured to trace, with the reader, each step of the varied argument, by which our author seeks to prove, that it is not to the dark recesses of remote antiquity we should resort for illustrations of the workings of nature, but rather to the scenes enacting even now before us. And having felt and admired the remarkable analogy which is shown to exist between the operations of nature at every period, whether early or late, whether exhibited in the organic or inorganic world, whether in the processes of destruction or of renovation, we acknowledged that our author had established much, if not all of his position, “that existing and now acting causes are sufficient to account for geological phenomena,” and recorded our opinion, that at least it would, ere long, be admitted by all, as it is now by many geologists, that in the causes which have operated at former, and in those acting in the present times, there is an identity of kind, though possibly a difference of degree, limiting therefore to that distinction the variations in modern geological theories. That opinion we find repeated by one of the ablest geologists of the day, the Rev. W. D. Conybeare, in

his "Report on the Progress, Actual State, and Uterior Prospects of Geological Science," read before the British Association, at its late Oxford meeting, and I cannot do better than quote his words:—

"Mr. Lyell's recent work, in itself sufficiently important to mark almost a new era in the progress of our science, may well close this imperfect survey. He has done well to call the attention of geologists to a generalized examination of the various changes still effected in our planet, by the physical causes in operation at the present day, under their precise actual conditions; because no real philosopher, I conceive, ever doubted, that the physical causes which have produced the geological phenomena were the same in kind; however they may have been modified as to the degree and intensity of their action, by the varying conditions under which they may have operated at different periods. It was to these varying conditions that the terms, a different order of things, and the like, were, I conceive, always intended to have been applied; though these terms may, undoubtedly, have been by some writers incautiously used. But as all are probably agreed, that the causes of nature are permanently the same in kind, however their operation at different periods may have been modified by the varying conditions under which they may have acted, it is an obvious consequence, that we shall be altogether unqualified to speculate on the former action of these causes, unless we are previously fully acquainted with their actual operation; this must ever be the great key to the analogies of their earlier geological operation; and he who has so ably extended our knowledge on this fundamental point, as Mr. Lyell has done, must ever be considered a most important benefactor to our science."

To so clear an exposition as this, from the pen of one rather inclined to underrate than to overrate the sufficiency of existing causes, we need not add one word; and assuredly the result it unfolds was, as we premised, to be expected, for taking first one branch of the subject, we may fairly say that there are few who still continue to doubt the strict analogy between granites, basalts, porphyries, and modern lavas, as igneous rocks, though there are many who yet think, that the vast extension of granite, as compared with modern lavas, points to some more general fluidity in the substance of the earth than now exists, and also, that the great elevation of so many ponderous masses, as in the form of mountain chains now project from its surface, can scarcely be explained by a reference to what they deem, in comparison, the puny efforts of the modern earthquake or volcano. This then is the present state of the question. The one

party consider that though the causes which formerly acted upon the earth, were similar in kind to those now acting, they were possessed of a vastly superior energy or intensity; for instance, that the temperature was higher, and in consequence, that more matter could be retained in a melted state; that in like manner the elasticity of pent-up gases would be augmented, and give rise to greater explosive efforts and more marked elevations. Or, if we follow Cordier in his views of the causes of earthquakes and volcanoes, we still find the decrease of the earth's temperature assigned as a reason for their supposed diminished energy. The other party, for Mr. Lyell is no longer alone, maintain, that the mean temperature of the solid mass of the globe may have, and probably has continued the same, though from the variations of sea and land, consequent on the uplifting of new, and the disappearance by subsidence of old, (phenomena fully established by geological facts,) the temperature at many points may have been reduced far below what it had once been; and further, that the same causes may and will produce similar and equally decided effects in future ages. The one think that the earth has gradually arrived at a state of quiescence, its temperature being nearly equilibrated between the loss by radiation, and the gain from the solar rays: and, that the occasional shocks of earthquakes, and eruptions of volcanoes are but feeble and expiring efforts of disturbing forces, once infinitely more vast and formidable than now. The other party maintain, that in the earthquake and volcano, as now manifested to us, we have a sufficient cause for all those effects which are attributable to such a class of agents. With them there is no alteration, no diminution, no increase, but an unvaried progression; no fearful period of intense and unusual violence, exhausting for a time the forces of nature in one amazing throe, but a succession of regular and moderate developments of its powers. First, at some remote epoch, commencing with the gradual uplifting of the submerged rocks and strata, and then continuing by fresh impulses to protrude them above the waters, and to raise them into mountain masses, as, vol. iii. p. 34,—“a series of subterranean movements might first give rise to small rocks and isles, and then by subsequent elevations to larger islands, by the junction of the former;” and so on, the appearance of new land being necessarily attended by the further elevation of the old.

Nor is it only in their views of igneous agency that this difference between the two geological schools appears; no, it affects equally their interpretation of the action of aqueous agents. The one find in the modern shingle bank, in the coral reef, and in the broken rocks which are buffeted by the waves

as they dash against the cliffs, types of the ancient conglomerates and gravel beds, of the madrepora limestones, and of the huge heaps of shattered fragments, which encumber the base of so many mountain precipices; and carrying back their mental vision to the time when the present surface was in great measure covered by the waters of the deep, they see oceanic currents sweeping over it, and hurrying along the detritus of its newly emerged cliffs, which yielded to the destroying waves beating as now against them. The other recur to the agency of the vast and sweeping wave of a mighty deluge, for an explanation of what are called diluvial phenomena, such as the accumulation of masses of gravel, the transport of boulders, and the denudation or removal of extensive strata. An extract from the volume before us, will mark more strongly this important distinction. Speaking of the origin of the English tertiary strata, our author, after commenting on a passage of Dr. Buckland's, which ends thus:—"The greater integrity in which the tertiary strata are preserved within the basins, has resulted from the protection which their comparatively low position has afforded them, from the ravages of *diluvial* denudation," proceeds, vol. iii. p. 284:—

"We conceive that the chalk, together with many subjacent rocks, may have remained undisturbed and in horizontal stratification, until after the commencement of the Eocene period, when at length the chalk was upheaved and exposed to the action of the waves and currents, it was rent and shattered so, that the subjacent secondary strata were exposed at the same time to denudation. The waste of these rocks, composed chiefly of sandstone and clay, supplied materials for the tertiary sands and clays, while the chalk was the source of flinty shingle and of the calcareous matter, which we find intermixed with the Eocene clays. The tracts now separating the basins of London and Hampshire, were those first elevated, and which contributed, by their gradual decay, to the production of the newer strata."

And again, when reasoning on the great denudation of the weald of Kent, vol. iii. p. 289:—

"It will appear from former parts of this work, that the amount of elevation here supposed to have taken place, is not greater than we can prove to have occurred in other regions, within geological periods of no great duration. On the other hand, the quantity of denudation or removal by water of vast masses, which are assumed to have once reached continuously from the north to the south Downs, is so enormous, that the reader may at first be startled by the boldness of the hypothesis. But he will find the difficulty to

vanish, when once sufficient time is allowed for the gradual and successive rise of the strata, during which the waves and currents of the ocean might slowly accomplish an operation, which no sudden diluvial rush of waters could possibly have effected."

Of these principles there are numerous illustrations in chap. 21 and 22, accompanied by most cogent reasoning; and in further support of the doctrine, that our inland precipices are no more than ancient sea cliffs, a doctrine which we have ourselves advocated, we shall transfer still more from those pages to our own. Having traced the chalk escarpment continuously along the southern termination of the north Downs, following it "from the sea at Folkestone westward to Guilford, and the neighbourhood of Petersfield, and from thence to the termination of the south Downs at Beachy Head," and described the strata in this precipice or steep slope, as cut off abruptly, Mr. Lyell observes, vol. iii. p. 291:—

"The geologist cannot fail to recognise in this view, the exact likeness of a sea cliff, and if he turns and looks in an opposite direction, or eastward, towards Beechy Head, he will see the same line of height prolonged. Even those who are not accustomed to speculate on the former changes which the surface has undergone, may fancy the broad and level plain to resemble the flat sands which were laid dry by the receding tide, and the different projecting masses of chalk to be the headlands of a coast, which separated the different bays from each other."

To the above, we might add many illustrations from the inland precipices or cliffs of our own island, such, for instance, as the steep or northern face of Cahirbarna, in Kerry, and the sandy terrace or level at its base, the true character of which is displayed by the river which has cut its winding passage through the loose materials: but we shall close this part of the subject by another example from the work before us, vol. iii. p. 209:—

"A few miles west from Dax, and at the distance of about twelve miles from the sea, a steep bank is seen running in a direction nearly north-east and south-west, or parallel to the contiguous coast. This steep declivity or brae, which is about fifty feet in height, conducts us from the higher platform of the Landes, to a lower plain which extends to the sea. The outline of the ground might suggest to every geologist the opinion, that the bank in question was once a sea cliff, when the whole country stood at a lower level relatively to the sea. But this can no longer be regarded as matter of conjecture. In making excavations recently for the foundation of a building at Abesse, a quantity of loose sand which

formed the slope was removed, and a perpendicular cliff exposed about fifty feet in height. The bottom of this cliff consists of limestone, which contains shells and corals of miocene species.* Immediately below this limestone is clay, and above it the usual tertiary sand of the department of the Landes. At the base of the precipice, are seen large partially rounded masses of rock, evidently detached from one of its strata. The face of the limestone is hollowed out, and weathered into such forms as are seen in the calcareous cliffs of the adjoining coast, especially at Biaritz near Bayonne. It is evident, that when the country was at a somewhat lower level, the sea advanced along the surface of the argillaceous stratum, which, by its yielding nature, favoured the waste and undermining of the more solid superincumbent limestone. Afterwards, when the country had been elevated, part of the sand fell down, or was drifted by the winds so as to form a talus, which masked the inland cliff until it was artificially laid open to view."

Nor let it be supposed that this striking, this graphic similarity, is at all affected by the circumstance of proximity to the sea, or that it would be difficult to carry the same illustrative reasoning into the most inland regions. Far from it, the step is in either case equally easy, for we have only to remember, that as every point of the present dry land has been (excepting of course the portions resulting from volcanic eruptions) covered by the ocean; and further, that as a very great extent of its surface has originated in the spoils, and in the operations of organic beings, capable alone of living in water; so is it clear to demonstration, that however remote from the present bed of the sea each inland cliff may now be, it must, in the course of those natural operations, by which it had been raised from the deep, have been exposed either for a long or for a short time to the angry surges of the struggling ocean. And we have dwelt at some length on this important truth and its consequences, because it is not one which usually attracts the notice it deserves. It is indeed a truth, which does not at first force itself on the mind of the casual observer; but when it is perceived, what delightful interest does it not spread over the contemplation of many of the objects around us, as under its influence they become marshalled, not into a wild and distorted, but into an easy, pleasing, and natural representation of each successive form of the earth's surface.

* *Miocene* and *Eocene*, are names given to subdivisions of the tertiary strata.

Outlines of Physiology and Pathology. By WILLIAM PULTENEY ALISON, M. D., F. R. S. E., Fellow of the Royal College of Physicians, and Professor of the Institutes of Medicine in the University of Edinburgh.

ANY work from the pen of Dr. Alison, carries with it in its author's name a sufficient recommendation. We wanted such a work as this before us to place in the hands of pupils, a work that gives them elementary instruction on an important branch of their science, and conjoins with such elementary instruction, a clear *exposé* and a candid criticism of what has been advanced by preceding writers. In two of the most important, and certainly the most beautiful departments of medical science, Comparative Anatomy and Physiology, there is in these countries an unaccountable deficiency, and we might, we are sure, safely predict, that whoever, with industry and zeal, shall enter on the cultivation of either of these fields, will, most certainly, confer important benefits on medicine, and in so doing, obtain high character and its rewards for himself. For the deficiency which at present exists in our cultivation and knowledge of physiology, the heads of our colleges and public institutions are much to be blamed, for with the single exception of one professorship, (that filled by Dr. Graves of this city, who supplies the want as far as the first rate abilities of an individual can,) there is not we believe in England, or in this country, a course of lectures devoted to physiology alone. It is true, physiology is advertised to be taught in conjunction with anatomy, but unless matters be very differently managed now from the modes of a few years back, physiology thus taught, is but a mere farce; the student's attention has been for months unremittingly fixed on the angles of bones, or the borders of muscles, and physiology, or the science of life, the attractions of which could alone have reconciled him to the loathsome labours of the dissecting room, is thrown in as a make weight at the fag end of the course. Such a cursory view of physiology (and we fear such is the view too generally presented) is just as unpardonable, as if a teacher of chemistry were to spend nearly all his time in giving elaborate descriptions of alembics, modes of powdering, sifting, &c., and leave all the beautiful principles and laws of the science to be jumbled together, and skimmed over as if they scarcely deserved notice. If continental schools in many points excel us, they have attained and they keep up their superiority, not by dry descriptive anatomy, on which all schools are probably pretty equal, but by the

superior physiological knowledge which they impart. We trust, for the credit of the profession of our own country, that a very short time will have elapsed, before there shall be established in every school, a professorship of physiology, with a course of lectures of at least equal length as those at present given on anatomy. We hope that in giving our readers some insight into the work before us, and some idea of the mass of information it contains, it is unnecessary to impress upon their attention the close relation which exists between physiology and the most useful and practical parts of medicine.

Physiology may, with propriety, be termed the science of life, or that science which teaches us the laws and properties of living bodies. Former writers on the science, (*vide* Bichat, Richerand, &c.) deemed it necessary to set out with giving a definition of their subject "life." We may be excused for making some observations on the futility of such an attempt. We observe in living bodies, certain changes or actions going on, which are inexplicable by any series of laws, save those drawn from a careful observation of the phenomena of those living bodies, and which laws apply to living bodies alone. We infer or admit that those laws, as they are constant and fixed, are the result of the operation of some one growing principle residing in the living bodies, in which those laws are observed, and to this unknown or hypothetical principle we give the name of life or vitality, just as from the observance of the action of masses of matter, we infer or admit, for convenience sake, the existence of a principle of gravitation, or in the action of atoms of matter on each other, the existence of a principle of affinity. But as we cannot define or give an idea of the nature of either of those principles, no more can we define or give an idea of the nature of life. To define it with one physiologist, to be "a collection of functions which resist death," or with another to be "a collection of phenomena or actions, which are constantly reproduced during a certain time in organized bodies," is just to leave us where we set out from, and to tell us that a living body is possessed of life, but we are as ignorant as before we had been made acquainted with such attempts at definition, of the nature of the something attempted to be defined.

Whether there be any such active principle in matter as gravitation, or in living bodies as life, or whether the phenomena which we refer to either, be merely a property given to each mass or atom, to be held for a certain period of time at the pleasure of the Author of Nature, is more than human intellect can ever know. All that we can ever know, and all that

is necessary for us to know is, the laws which govern the action of bodies, calling, for convenience sake, one group of laws observed in the action of bodies under certain circumstances, laws of gravitation, another group of laws observed in other circumstances, laws of affinity; and in like manner, the laws discovered by our observation of those bodies which we call living, the laws of life.

“The word life, as commonly used, does not denote an individual fact, nor a simple idea, and cannot, therefore, be *defined*. It is applied to a certain assemblage and succession of phenomena, which are seen in a great variety of the objects that surround us, and distinguish them from the other objects of our senses. When these phenomena are examined throughout the whole of nature, it is found that the most general and characteristic of them is, the continued appropriation and assimilation of surrounding matter, which we call *Nutrition*; a process which maintains a certain definite structure called *Organization*,—which originates, in all cases that can be satisfactorily observed, by *Generation*,—and terminates by *Death*.

“Some have conjectured, that the phenomena of life, as they are seen only in bodies more or less organized, depend merely on the circumstance of organization; but when we inquire how organization has been effected, we find that it implies in every instance, where we can observe it, the previous existence of vitality; and therefore must be regarded as one of its effects, not as its cause.

Others have formed the supposition of a *material substance*, such as an ethereal or subtile fluid, superadded to organization during life, and producing the phenomena of life; but this idea is both unsupported by evidence, and useless in the explanation of facts.”

Dr. Alison classes the phenomena of living bodies under three great heads:—“1st. Those of *vital contractions*, by which the visible movements of living animals are chiefly effected. 2nd. Those of *vital affinities*, by which the chemical changes peculiar to living animals are determined, and their physical structure maintained. 3rd. Those of *nervous actions*, by which the physical changes in living animals are placed in connexion with mental phenomena, and subjected to the control of mental acts.”

The first of these classes, or vital contractions, includes not alone the contractile power of common muscular parts, as seen best in voluntary muscles under the influence of the will, or upon the application of a stimulant, but also the power of vital contraction existing in tissues, through which various circula-

tions are carried on, and in which we are not able to detect any muscular structure. Vital contraction is, as we have just observed, best seen in a muscle on the application of any agent, which has the property of exciting its contraction, and which is hence called a stimulant, and the property which the muscle possesses of taking on a contractile action on the application of a stimulant, is called its irritability.

“ When this contraction takes place, the filaments constituting the muscular fibres assume a zigzag form, the angles formed being always at the same points, and being generally obtuse, but in the case of very forcible contraction, acute; the fibres become rigid and elastic, and it would appear that they have a vibratory motion during the continuance of the contraction, they often appear to swell out towards their centres, but experiments shew that their real bulk is not altered; the change being in the relative position, not in the size or distance, of their ultimate particles.”

When this property, irritability, is called into action, it generally alternates with relaxation of the muscular fibres. There is, however, a variety of vital contraction, to which the name of tonicity is given, and which differs from irritability in this respect, that the contractile property or power continues without any alternate relaxation. We have examples of tonicity in the stiffening of limbs after death, in the persisting contraction of the sphincter muscles during life, and in the retraction of the ends of a cut muscle.

The source or origin of the contractile power of muscles, or of their irritability, has been a fruitful cause of dispute among physiologists. The different opinions may be ranged under three theories. The first, Haller's opinion, supposes the contractile power of muscles to reside in themselves, to be an inherent power: the second supposes the muscles to possess no contractility or irritability of their own, but to derive all from the brain or nervous system: and the third, admitting muscular fibres to be endowed with contractility *per se*, nevertheless assert, that no stimulus can excite that contractility, unless by first acting on the nervous filaments which supply the muscular fibres. Dr. Alison supports Haller's opinion, observing, that “ the final causes of all endowments bestowed on nerves in the living body in relation to muscles, appear to be, not to make muscles irritable, but to subject their irritability in different ways, to the dominion of the acts and feelings of the mind.”

To the theory which supposes muscles to derive all their power from nerves, Dr. A. makes the following objections:—

" 1. Indications of a vital power of contraction are a much more general fact in nature than the existence of nerves, being seen not only in the lowest class of animals, where the existence of a nervous matter is still doubtful, but also in vegetables.

" 2. The human foetus has often come to the full size (implying long continued exertion of contractile power in the organs of its circulation) without a brain, and sometimes without either brain or spinal cord; and in all cases, during the growth of the foetus, the formation of vessels precedes that of nervous matter, and the organs of circulation are in full operation, when the larger masses of the nervous system are still very imperfectly developed.

" 3. The involuntary motions of the organs of circulation have been found by many physiologists to continue vigorous, even in warm-blooded animals, after the removal of the brain, or even of both brain and spinal cord, provided that the exposure of the blood to the air is secured, by the substitution of artificial for the natural respiration (which being undoubtedly and directly dependent on the brain, necessarily ceases on its removal.) And in these circumstances, when circulation, after some hours, does come to a stand, it is chiefly, as it would seem, because the artificial is only an imperfect substitute for the natural respiration.

" 4. No interruption of the contractions of the heart, or other strictly *involuntary* muscles, has ever been produced by cutting the nerves, by which they are immediately supplied.

" 5. Although the muscles destined to voluntary motion are no longer moved by the will, after the section of their nerves, they continue thereafter to shew their irritability, on the application of stimuli to themselves, or to their nerves below the section, as long as their nutrition and organization continue pretty entire; and contractions may be excited by direct and continued irritation in the muscular fibres, fully as long in muscles of which the nerves have been cut, as in those, the communication of which with the brain and spinal cord, through the nerves, is entire."*

And against the theory, which, admitting muscles to possess contractility, nevertheless asserts, that a stimulus, in order to excite that contractility, must always act through the medium of the nerves of the muscle excited, he brings forward the following argument :—

" Again, to the *second* of the theories above stated, it appears a sufficient objection to state, that our only reason for supposing an intervention of nerves to be concerned in muscular contraction, is the excitation of that contraction by stimuli applied to nerves. But a conclusion which is rested on this fact, must be limited to the cases

* Wilson Philip, *Experimental Inquiry*, &c. p. 99.

in which this fact holds good. Now, there are *many muscles* (viz. all or almost all those that are destined to *involuntary motion* only,) which, although exceedingly irritable, *are not excitable by mechanical irritation of their nerves*. Even Galvanism, applied exclusively to the nerves of these muscles, has generally failed to excite them; and in the instances where galvanism, so applied, has had some effect, it appears probable that the nerves acted only as conductors of the galvanism to the muscular fibres themselves. When experience shews, that some muscles are excitable by irritation of their nerves, and others not, we cannot acquiesce in the proposition, that nerves furnish a condition essential to the irritation and vital action of muscles in general."

The doctrine of accumulation of irritability from rest, is now so universally exploded, that we need not make any observation on its absurdity; it is sufficient to say, that it is pure hypothesis, unsupported by a single fact. Under the first head, vital contraction, is included, an account of the circulation, of its course, and of the proofs which support the account universally taught of its course. This elementary part is clearly and succinctly given. We must notice, however, a slight anatomical error of description, which is pardonable, if its frequent occurrence in nearly all descriptions be an excuse for it, we mean calling the inferior or flattened surface of the heart, or that which lies upon the diaphragm, the posterior surface. This error is perpetuated by the examination of the heart in the living inferior animal, where the heart lies in the thorax, almost parallel with the length of the sternum, and by all the dried injected preparations of the vascular system we have ever seen, in which the heart is invariably allowed to hang in a depending position, a position it can never assume in the living human being. In man the surface which Dr. Alison and most describers call the posterior surface, has no claim whatever to such a name, but is really the inferior surface flattened by its lying on the diaphragm, and no more entitled to the name of posterior surface, than the sole of the foot to a similar appellation. A case occurred under our observation, not very long since, which clearly exemplified this point in anatomy. Two men quarrelled, and one of them standing on a staircase, a few steps lower down than the other, thrust upwards with a sharp pointed shoemaker's knife, and stabbed his antagonist a little to the left of the ensiform cartilage. The man who received the wound, died in a few minutes. On examination it was found, that the pointed knife, in the upward thrust, had passed up through the flattened surface of the right ventricle, and wounded the corresponding internal surface above, which

could not possibly have happened, if the flattened surface were the posterior surface, as generally described. Dr. Alison assumes as granted, an active dilating power in the heart, and looks upon it as one of the causes of drawing the blood into this organ from the veins. We are sceptical on this point, but at most it is one still open for examination, and should have been laid down as such. Without going into any lengthened examination on the point, we think there is in the pathology of the heart a powerful argument against the possession of such an active dilating power. We know that the ventricles and auricles become hypertrophied, when they have resistance to overcome in forcing forward the current of blood, for instance, the auricles are hypertrophied when there is narrowing of the auriculo-ventricular opening, and the ventricles are hypertrophied when there is obstruction at the arterial orifice: this is in accordance with the well-known law, that muscular fibre becomes thickened and strengthened where increased exertion is repeated. If the muscular fibres of the heart possessed, as Dr. Alison and others supposes, a power of actively dilating and exerting a suction power, the consequence should be, that when the auriculo-ventricular opening is narrowed, the ventricle, from its increased exertion to dilate and suck blood into its cavity through the narrowed opening, ought to become hypertrophied, in accordance with the law mentioned above, but it never becomes hypertrophied from such a cause, and this absence of hypertrophy is, in our minds, a strong argument against the heart possessing an active muscular dilating power.

On the agency of the arteries in carrying on the circulation, Dr. Alison leans to the opinion, that both large and small perform a very secondary part, compared with the heart. He throws out a supposition, and one very probably true, that in carrying on the capillary circulation, there is, in addition to the agency of the heart, and contractile power of the arteries and capillaries, a third power of whose nature we are yet ignorant, but governed by laws such as regulate the movements of fluids in vegetables, and in the lowest classes of animals, where these movements take place with rapidity and without the intervention of heart or circulating vessels, and of which the researches of Dutrochet have furnished us already with numerous illustrations. To this new power he gives the name of vital affinity, and to the same power or property he refers the phenomena of secretion and nutrition. A very interesting question in physiology at present is, whether the various substances which constitute the solids of the living body are manufactured, if we may use such a metaphor, by the vessels or organs of each particular part, from the supply of nutritive fluid which it re-

ceives, or whether the various substances constituting the living body and its excretions, all exist ready formed in the blood, and are only evolved from it by the vessels or capillaries of each particular part, or in shorter words, whether the formation of the proximate principles of a living body, is a part of the process of assimilation or of secretion. On this question Dr. Alison comes to the conclusion, which we believe would constitute the true answer to many similar disputed points, namely, that neither view is exclusively true. The arguments *pro* and *con* are clearly and fairly put, we shall give them in Dr. Alison's words, because they contain much information, and afford a good specimen of Dr. Alison's style:—

“ It is an important question, whether the different textures and secretions of the body are actually *formed* from the circulating blood, at the parts of the body where they appear, or are formed in the blood itself, and only *evolved* or separated at these parts. The following facts seem to favour the last opinion.

“ 1. It is a general law of living animals, and probably of vegetables* also, that all nourishment received from without must be mixed with secretions of the organized body itself, and so far elaborated in its interior, before it is applied to the support of any part of the organization; which makes it probable that an important part, at least, of the process which fits it for giving that support, is completed before it reaches the organs where it is so applied.

“ 2. As the blood contains, or shews after the simplest chemical processes, fibrin, albumen, muco-extractive matter resembling osmazome, water, oil, the phosphates of lime and magnesia, and most of the other salts found in the secretions, it appears fitted to yield, by mere separation of its constituent parts, by far the greater part of the substances found in the textures and secretions of the body.

“ 3. There are various instances, in disease, not only of textures widely extended over the body, such as bone and fat, being deposited in unusual situations, but also of substances usually secreted in special organs only, such for example as cholesterine, being deposited in parts distant and very different from those where they are commonly found; and even independently of structural disease, there are various well authenticated cases on record, where secretions, especially those of urine and of milk, have been thus established, and passed off for some time, “*per aliena cola.*”†

“ 4. It has been ascertained, first by Prevost and Dumas, that when the secretion of urine is suppressed by extirpating the kidneys of animals, the urea, which is the characteristic part of that secre-

* See Knight, Philosophical Transactions, 1805.

† Setting aside, as inapplicable here, many of the cases recorded under this head by Haller (Elem. lib. 7. c. l.) there remain several which seem to justify

tion, may, after a time, be detected in the blood; and the same has been found in the human body, in some cases where the secretion of urine has been much obstructed by disease, whether functional or organic, of the kidneys.*

"5. It is often observed, in cases of disease resulting from the introduction, or retention within the body, of noxious or unassimilable matter, that this morbid matter, certainly mixed with the blood, is evolved from the circulation at certain parts, or in certain textures rather than others; which implies a peculiar relation of foreign matters circulating with the blood, to the vital properties of peculiar textures, and makes it probable, that there will exist similar relations of textures to the different natural constituents of healthy blood.

"But, on the other hand, it is to be observed,

"1. That a conclusion resting on observations made on the secretions of urine, bile, and milk, which are destined to excretion, is not necessarily applicable to the case of those formations from the blood which are destined to useful purposes in the animal economy.

"2. That it is doubtful whether the fat of the animal body or the nervous matter, can be recognised in the blood, and certain that the gelatin, which is easily procured in abundance from any animal textures, cannot be recognised there; and several of the other elements of the animal textures, found in small quantity in the blood, may be supposed to be the products of *absorption*.

"3. That in insects and zoophyta, where secretion and nutrition are seen in their simplest forms, the general nourishing fluid, formed and contained in one internal cavity, appears to furnish a variety of products very different from itself, by a process hardly more complex than mere transudation through a living membrane.†

"These facts lead us to believe, that, although many of the products formed in the living body are so far elaborated in the blood itself, yet, in various instances, a material change is effected in these products at the time and place of their escape from the circulation."

The chapter devoted to respiration and to the examination of the opposing opinions on the formation of the carbonic acid of expiration, is worthy of an attentive perusal. The notices of experiments of preceding writers are short, but Dr. Alison takes care to atone for this by giving copious references to the writings from which he has extracted them. On those parts of the book devoted to a consideration of the nervous system, we can-

this observation; and various similar cases have been recorded since, e. g. in Magendie's *Journal de Physiologie*, vol. vii.; and *London Medical and Physical Journal*, June, 1828.

* Bostock's *System of Physiology*, vol. iii. p. 412. Christison, *Edinburgh Medical and Surgical Journal*, vol. xxxii. pp. 271 and 274.

† Cuvier, *Leçon*, 23, sect. 2 art. 5.

not speak so favourably as of the other portions. We should have expected in this work, a clear and pretty full statement of the various and still conflicting opinions, relative to the functions of different parts of the nervous system, and this is not given. Of some of his criticisms we do not think very favourably: we cannot for instance say, that while he objects to the theory as vague and indefinite, which supposes the ganglionic nerves to preside over the involuntary motions and organic functions in general, he gives us a clearer notion of their functions by saying, that "the object of these nerves is to bring these functions and the actions of certain voluntary muscles, *under the dominion of the involuntary acts of the mind*," p. 278. There is in the work before us, too much of metaphysical physiology, and we think there would be an improvement in subsequent editions, by weeding out much of this, and substituting for it the physiology of experiment and observation. Outlines of pathology form a part of the volume, we shall bring these and some works in a similar line from other authors, before our readers in the next Number.

Illustrations of the Elementary Forms of Disease. By ROBERT CARSWELL, M. D. Professor of Pathological Anatomy in the University of London, &c. &c. Fasciculus first.—Tubercle. Royal 4to, four coloured Lithographic Plates, with accompanying Letter-press. London, Longman's, 1833.

THE first number of this important work has appeared, and we hail it with no small degree of pleasure. We have had numerous works already on Morbid Anatomy, the object of which has been to combine graphic illustrations with written description, but we can safely say, that this one, if continued in the style of the first number, bids fair to surpass them all in accuracy of description, fidelity of representation, and elegance of execution. The character of the author as a profound and philosophical pathologist is so high, that we feel the recommendation of his writings to be unnecessary. On the subject of the plates we may remark, that they bear the stamp of truth in every line and tint; there is no caricaturing of nature; none of that exaggeration of colouring, which is so great a fault in other works of this description. They are manifestly not the productions of a mere artist, but of one whose scientific attainments have stamped them with the impress and value of reality. We feel that we are safe in giving this national work our warmest recommendation.

SCIENTIFIC INTELLIGENCE.

CHEMICAL AND PHYSICAL SCIENCE.

Temperature of Thermal Waters.—M. Boussingault read a paper on the temperature of the thermal waters of the Cordillera of the Andes. The explanation proposed by Laplace, of the heat of the mineral waters, is confirmed by a crowd of facts observed on different points of the Cordillera. M. Boussingault thought he remarked that in the chain of the coast of Vénézuéla, the temperature of those waters is as much less as their absolute height is greater. For example the warm water of Las Trincheras, near Porto-Caballo, which is almost on a level with the sea, presents a temperature of 97° centigrade; that of the source of Mariara, at the elevation of 476 metres, is but 64° ; and that of the source of Onoto, situate at an elevation of 702 metres, is but $44^{\circ}.5$. Yet in the vicinity of the volcanoes, this regularity is no longer observed in the decrease of the thermal waters. It appears that under this circumstance, the local cause producing the volcanic phenomena has a marked influence on the temperature of these waters. It becomes then very interesting to investigate whether the thermal sources have their origin near the volcanoes. To come at the solution of this question, it is necessary to submit to chemical analysis the mineral waters near the volcanoes, and particularly to seek whether the gases which they contain are the same as those found in the craters. If so, it would be a strong reason for supposing that the thermal waters have been in contact with the matters which exist in the volcanic fires, and determining the saline substances dissolved in the mineral waters would acquire a new degree of interest, since these salts must be considered as soluble products existing or formed in the interior of the volcanoes. After these considerations, the author has undertaken the analysis of the thermal waters, which he met with in his travels. The result of his researches is, that the gases existing in the thermal waters, adjoining volcanoes, are of the same nature as those found in the craters of these volcanoes, to wit, carbonic acid, and hydrosulphuric acid gases. Whence M. Boussingault concludes that it is probable that the warm waters of the trachytic (*trachytique*) country near the equator, owe their temperature to the subterraneous fires, and that it is quite natural to think that the salts dissolved, or contained in these waters, arise from the interior of the volcanoes.

The memoir of M. Boussingault concludes by some researches as to whether the temperature of the thermal waters of the Cordilleras is subject to variation. The results of these observations give an affirmative solution.

In 1800 M. de Humboldt found the temperature of the source of Mariara at 59° , 3: in 1823 it was, according to the observations of MM. Boussingault and Rivero at 64° . This difference of 5° , 3, cannot be attributed to an error of the instrument, for the thermometrical observations which these two travellers made at Guayra and at Caraccas, coincide perfectly with those of M. de Humboldt. On the other hand the researches made by them on the temperature of the waters of Las Trincheras, are also in support of the same opinion. The waters of this source issue from two small basins placed by the side of each other, dug out in granite. The larger has a capacity of about two cubic feet. M. de Humboldt gives 92° , 2 for the temperature of Las Trincheras. Twenty-three years after MM. Boussingault and Rivero ascertained that the temperature of one of the basins was 92° , 2, and that of the other 97° . These observations, as also those of M. de Humboldt, were made in the month of February. Thus in the space of 23 years, it appears that the temperature of the sources of Mariara and of Las Trincheras was raised several degrees. The narrator remarks, that during the interval which separates the observations of M. de Humboldt from those of M. Boussingault, the entire coast of Venezuela was shaken by the great earthquake of the 26th March, 1812, which destroyed the city of Caracas, and several of those situated in the eastern Cordillera.—*Arch. Gen. Mars*, 1833.

On the simultaneous Presence of Prussian Blue, and of a Saccharine Matter in a particular Variety of human Urine, by M. Cantu, Professor of General Chemistry applied to the arts, in the University of Turin.—The presence of prussic acid in human urine, secreted during a morbid state of the animal economy, was already announced upwards of forty years ago by Brugnatelli, and MM. Moyon and Julia-Fontanelle observed a few years back that of prussian blue and saccharine matter.

As the knowledge of this extraordinary fact may give rise to new observations and farther experiments on the part of physicians and chemists, and influence the progress of the theory and practice of medicine, this subject seemed deserving the attention of the Royal Academy.

I shall briefly state the experiments which I made on this subject, the results I obtained, and the inferences which I consider it warrantable to deduce from them.

The urine in question was sent me by Dr. Bernetti, member of the College of Medicine. According to the intimations which he had the kindness to communicate to me, it came from a little girl about eight years old, who complained of no indisposition, except some colicky pains occasionally felt in the epigastric region, a little before she experienced a necessity to void urine. It is necessary to

remark that she was not at that time subject to any medical treatment, and that she used nothing but ordinary food and drink, it being merely the extraordinary circumstance of the blue colour of the urine which excited the attention of the parents, and made them consult the physician on so strange an occurrence, from which they dreaded, not without reason, some serious results.

This urine immediately when passed was of a blue colour, similar to that of the solution of indigo in dilute sulphuric acid, at least such was the colour of the urine voided at night; that passed by day was of a less deep colour, it bordered slightly on green, merely because being more watery it contained less prussian blue, on which its blue colour depended, as we shall see presently: scarcely could the odour and taste of ordinary urine be distinguished in it, but there was clearly perceived the odour of syrup of sugar, and a decidedly sweetish taste, similar to that of the urine in *diabetes mellitus*.

A portion of this urine put into an open vessel, and left to the action of the air at a temperature of from 13° to 18° Reaumur, began to assume a less deep shade after about twelve hours, it then became greenish, and finally acquired a yellow citron colour, throwing down some flocculi of mucous matter of the same colour. In the mean time whilst these changes were becoming manifest, there was developed a slight ammoniacal odour, and re-agents indicated the presence of an alkali.

From these phenomena it may be inferred that the urine experienced a partial decomposition which gave rise to the ammonia: this latter by decomposing the prussiate of iron contained in the same urine dissipated its blue colour. The urine thus deprived of its colour, and altered in its nature, still left to the influence of the same causes, lost gradually the ammoniacal odour, and in the space of two days acquired a sour smell, slightly alcoholic, and its blue colour appeared somewhat less intense than before.

Hence it appears that the farther decomposition of the urine gave rise to the formation of acetic acid, and to some alcohol; and that the ammonia having been saturated by this acid, in proportion as it was formed, the prussic acid reacted on the oxide of iron, and produced by a natural consequence the blue matter of the urine, or the prussian blue. The blue urine, not altered, put into a well-closed glass vessel, in a medium whose temperature did not exceed the sixth degree of Reaumur, was kept for eight days without sensibly losing its colour. But the vessel being uncorked, and left to the influence of the air in a higher temperature, its colour was considerably weakened, and in the space of twenty-four hours the urine was entirely discoloured, and acquired an ammoniacal odour. But by continuing under the same influences, it presented the same phenomena already described, that is, it recovered its blue colour and a sour smell, slightly alcoholic, one may then deduce the same consequences. Another portion of the blue urine not altered, and to which there had been added some centiemes of sulphuric acid, left to the action of the

air, under a temperature of from 13 to 16°, kept its colour for 15 days, without disengaging any ammoniacal odour.

This experiment proves that the presence of the sulphuric acid opposed the so easy decomposition of the urine, and the consequent production of ammonia, and that even when any was formed, it must be entirely saturated by the sulphuric acid, with which it was brought into contact, and thus prevent the alteration of the blue colour.

The urine in question, a little time after being voided, did not produce any sensible change either on turmeric paper, nor on that of turnesol, that is, it manifested neither alkaline nor acid properties.

Sulphuric, nitric, muriatic, and acetic acids, poured on this urine, in a quantity sufficient to communicate to it well-marked characters of acidity, did not sensibly diminish its blue colour. Even chlorine added in a small proportion did not produce in it any effect.

On the contrary, with the alcalics, that is to say, with potass and ammonia, all the blue colour was destroyed, and the liquor assumed a yellow colour, similar to that of common urine, and of that which having been at first blue became yellow by the action of ammonia, the result of the spontaneous decomposition, as we have already shewn. On pouring the above-mentioned acids in sufficient quantity on the urine made yellow by potass or ammonia, the blue colour immediately re-appeared, an effect which also took place by the contact of the same acids with the urine made yellow by spontaneous decomposition, that is, by the action of the ammonia produced in the urine in this circumstance.

The blue urine, before being altered, when subjected to the action of the fire, and carried nearly to boiling, scarcely diffused the odour peculiar to ordinary urine, but there was distinctly perceived that of a solution of boiling sugar: this odour became more and more perceptible, in proportion as the liquor became concentrated, and particularly when it approached the consistence of a syrup. During this process the blue colour was not sensibly changed.

The residue arising from the evaporation of the urine, being carefully examined, presented the principles belonging to urine in the natural state: but a comparison being made of the proportions, the latter were found in infinitely smaller quantities in the urine now in question: the urea and uric acid existed in it in very small quantities: instead of it there was in it prussiate of iron, and a saccharine matter similar to that afforded by the urine in *diabetes mellitas*.

A small portion of the same residue being thrown on burning charcoal, diffused an ammoniacal odour; but that of burned sugar was particularly distinguished in it. Another portion of the same residue being distilled in a glass retort afforded slight traces of sub-carbonate of ammonia, but more particularly the products yielded by vegetable substances, when treated over fire in close vessels.

The carbonized matter, which remained in the retort, was subjected to the action of muriatic acid: the solution being filtered and tried with prussiate of potass, and with infusion of galls, yielded

with the first re-agent a deep blue colour, and was sensibly blackened by the second, which proves that the quantity of prussian blue contained in this particular urine was considerable. The different experiments just now described clearly prove in this secretion the simultaneous presence of prussian blue, and of saccharine matter. However, to remove all doubt, I took another portion of the syrupy residue of this urine, and I diluted it in a sufficient quantity of distilled water; by rest there was precipitated a powder of a blue colour, which being separated from the liquor by decantation, and well washed, acquired a purer blue; when afterwards subjected to the action of the alkalies, acids, chlorine, and other reagents, it gave precisely the same results as the prussiate of iron.

This being done, I poured into the supernatant liquor some subacetate of lead in a slight excess: by this means all the organic substance was precipitated, except the saccharine matter which remained in solution in the water; then there was made to pass through the filtered liquor a current of sulphuretted hydrogen in order to precipitate all the oxide of lead: the liquor again filtered was evaporated to the consistence of a thick syrup; there was then obtained a substance of a white yellow colour, of a well marked sweet taste, which, when put on burning charcoal, diffused the odour of burned sugar: and treated with nitric acid presents all the phenomena and all the results which saccharine matter generally presents with the same agents. It appears to me that from all these results we may deduce the following corollaries:

1°—That this urine contains prussian blue, and a saccharine matter, similar to that met with in the urine of *diabetes mellitus*.

2°—That the blue colour of urine may also depend sometimes on the presence of another substance discovered by Braconnot, or designated by him under the name of *cyanorine*: but that it is without good reason that this learned chemist raises doubts with respect to the discovering of prussiate of iron in blue urine, a fact announced by Julia-Fontanelle, in the *Archives Generales de Medicine for the year 1823*.

3°—That probably the extraordinary presence of these substances, that is, the prussiate of iron and saccharine matter, may develop itself anew by reason of the *innormality* (*innormalité*) of the secreting function of the kidneys, which, according to the experiments of Wollaston, may be regarded in some measure as a chemico-galvanic process.

4°—That free prussic acid having been discovered by Brugnatelli, in the urine of a dropsical patient, prussiate of iron by Fourcroy in the blood of an hysterical woman, prussiate of iron in the urine by Moyon and by Julia-Fontanelle; that a similar blue matter having been observed by Reisel in the sputa of a woman affected with pneumonia accompanied with frequent vomiting: that the same phenomenon having presented itself to Dolxi, to Mogi, and even to Julia Fontanelle, in the perspiration of other individuals labouring under nervous affections, it seems reasonable to think, that in the animal

economy, under a morbid condition, the prussic acid may be engendered more frequently than has been hitherto imagined : but that if the circumstances are rare in which this effect becomes the cause of serious disturbances in the system, this must be imputed to the presence of some bases capable of neutralizing it, and of paralyzing its deleterious effects.

5°—That in comparing the symptoms and organic lesions which present themselves in cases of poisoning by prussic acid, and in the *cholera morbus* of India, it appears to me no abuse of analogy to state the opinion, with other physicians, that prussic acid performs some part in this terrible malady. This idea moreover is in accordance with all that has been hitherto written by Rossi on miasmus, and even on the Indian cholera.—*Journal de Pharmacie*, April, 1833. (Extract from the Memoirs of the Royal Academy of Turin.)

On the State of Mercury in Citrine Ointment.—M. Cédie, after passing in review the works hitherto published on the subject of which he treats, establishes it as a fact, that the mercury is in a state of protonitrate in recently prepared citrine ointment, whilst ointment prepared for a long time contains merely metallic mercury minutely divided, which imparts to it the greyish colour.

He has verified these facts by treating this ointment with sulphuric æther. In the first case he separated from it a white powder, possessing the properties of protonitrate of mercury, and in the second a grey powder, which is nothing but minutely divided mercury. M. Cédie admits that the deuto-nitrate of mercury employed in the preparation of citrine ointment is first brought to the state of protonitrate by the deoxidizing power of the fat, and that, this effect continuing, the salt is entirely reduced. The second effect would be, according to M. Cédie, accompanied by a disengagement of nitrous gas, an idea which he grounds on the destruction of the characters of a paper ticket placed in the upper part of the vessel where he keeps the citrine ointment. This gentleman remarks, and with good reason, that the mode of action of recently prepared ointment, and that of long standing, cannot be the same, and he draws the attention of practitioners to this point.—*Ibid.*

On the Preparation of Iodic Acid, by M. Boutin.—Attached for some time to the Museum of Natural History, to prepare the course of general chemistry, of which M. Gay Lussac is Professor, and for this course having had occasion for a certain quantity of iodic acid, I endeavoured to obtain it by following successively the different means hitherto published.

I found that the oxide of chlorine presents the two-fold inconvenience of never being without danger, and of furnishing a very trifling quantity of acid. The three processes which Serullas has given in the *Annal. de Chim. et de Phy.* tom. xliii. p. 127 and 217, and in tom. xlv. p. 63, do not always enable one to procure it with as much ease as this celebrated chemist has asserted, at least for any one

who is not well practised in chemical manipulations. A note of M. O'Connell, printed in the 49 tom. of the *Ann. de Chim. et de Phys.*, in which he remarks the formation of iodic acid by the action of nitric acid on iodine, induced Serullas to repeat this experiment: but he observed that the employment of nitrous acid was preferable. Then having myself repeated these experiments, I obtained such results, that, by introducing certain modifications which I am going to point out, I have arrived at an excellent process for obtaining iodic acid.

Thus, to procure iodic acid, and in considerable quantity, let a mixture of nitric and nitrous acid in the proportions of eight parts of the former, and from one and half to two of the second, be made to re-act on *recently precipitated** iodine. The course to be followed in this operation consists merely in taking a matrass with a long neck, of a certain size, mounted by a long tube of about two lines in diameter.

Then place in the matrass the quantity of precipitated iodine which you wish to convert into iodic acid, pour on it the half or two-thirds of the quantity of the acid mixture already mentioned: the operation being thus arranged, subject it to a gentle heat. Instantly there appear thick vapours of a very deep brown red: the iodine comes to be condensed in the highest part of the matrass, as well as in the neck and in the tube which surmounts it, a circumstance which renders it necessary to shake the matrass from time to time, and even to incline it so that the liquid may reach the tube; lastly to detach the iodine which is volatilized, and which falls back again into the matrass; for without this precaution the tube might be obstructed. After some moments of re-action, we see appear at the bottom of the matrass small white grains, which are nothing but iodic acid already assuming a crystalline form: we thus continue the operation until the iodine has disappeared, and on adding by degrees in small portions the quantity of the acid mixture reserved towards the end, the vapours become less intense, and finish by disappearing altogether: then we pour the entire into a porcelain capsule, or, if we wish to collect the nitric acid, we distil in a retort the two-thirds of the liquid: what remains is of a dirty yellow, and would consequently alter the whiteness of the iodic acid, if we did not take care to decant it. Then dissolving this acid in a sufficient quantity of distilled water, we filter and evaporate the solution which is still a little coloured: when it is very concentrated, we add to it once or twice its volume of pure nitric acid: the iodic acid is immediately precipitated.

We remove the liquid part, which is slightly rose-coloured, and wash the precipitate of iodic acid once or twice with a little nitric acid: again re-dissolving the precipitate in three times its weight of distilled

* I here direct precipitated iodine, in order to be certain of its purity, and particularly because its extreme division determines much more readily the formation of iodic acid.

water, and adding to the solution the two-thirds of its volume of pure nitric acid, we then evaporate to dryness in a porcelain capsule placed in a sand-bath: by these means we obtain very beautiful and perfectly crystallized iodic acid.—*Ibid.*

Analysis of the Saliva, by M. Guibourt.—In 1831 Dr. Rayer was called in to attend a lady 24 years of age, of a nervous constitution, a gentle disposition, but very ardent, having no occupation but such as a great fortune gives or promises, and who for several years experienced, at irregular intervals of thirty, forty, or fifty days, a very copious attack of Salivation.

This young woman, though several years married, had no children: she is accustomed to sleep but little, and occasionally experiences fatiguing want of sleep, and distressing feelings which she cannot get over. Menstruation is regular, but small in quantity: at the accession the appetite is completely lost during 24, 36, or 48 hours, whilst the salivation lasts: the patient takes no nourishment; thirst inconsiderable; some mouthfuls of sugar and water suffice to quench her thirst, for the entire duration of this discharge, which is never preceded by shivering, pain, or swelling of the salivary glands, and never accompanied by fever or other appreciable morbid phenomena. The mucous membrane of the mouth is neither redder, nor more sensible; that during the intervals there is sometimes merely a sensation of constriction in the pharynx.

These attacks of salivation taking place at irregular periods, her health being tolerably good during the interval, it was thought fit to try the administration of quinquina to prevent them. At the end of one of these attacks of salivation, Mr. Rayer tried, without succeeding, to arrest it by opium, as recommended by Hunter. After another, he tried the quinquina without any advantage. At last, by means of subcarbonate of iron, given for several months, he succeeded in rendering these periodical salivations less frequent, but without perceptibly modifying their duration or diminishing the secretion of saliva, which then flows in such abundance from the mouth, that he has collected several pints of it in twenty-four hours.

Chemical examination.—Three chemists have principally directed their attention to determine the nature of the human saliva; but their labours, too concise and sometimes contradictory, still leave us in doubt with respect to the composition of this secretion. According to Fourcroy the saliva is composed of a very fine animal mucilage, nearly insoluble in water, of a very small quantity of albumen in solution, of muriate and phosphate of soda of ammonia and lime, (*Système des connaissances chimiques.*) According to Dr. Bostock, the saliva contains coagulated albumen, (the insoluble mucilage of Fourcroy,) some dissolved mucus, some muriate of soda, and phosphates of soda, and of lime; and farther, he observes that the saliva he used in his experiments was acid. And lastly, according to Berzelius, the saliva holds in suspension mucus which can be separated by diluting it in water, allowing it to rest, and then decanting it; it holds in solu-

tion, another animal substance which is peculiar to it, and which may be obtained by first treating the dried saliva with alcohol, in order to dissolve the alkaline chlorides and lactates; treating the residue with alcohol acidified by means of acetic acid in order to dissolve the free soda: then treating it by water which dissolves the particular substance, and leaves the mucus. According to the same chemist, this particular substance evaporated to dryness, leaves a transparent mass, which is dissolved anew and with ease in cold water. This solution is not precipitated by acids, subacetate of lead, chloride of mercury, or tannin; it is not disturbed by ebullition. The mucus is insoluble in acids, and does not yield to them any phosphate of lime: it is dissolved, on the contrary, in a caustic alkali, and precipitated by acids; it leaves a considerable quantity of phosphate of lime by calcination. M. Berzelius infers from these three combined facts, that the phosphate of lime does not exist formed in the saliva, and that it is a product of incineration.

The saliva yielded to this celebrated chemist—

Water,	992.9
Particular matter,	2.9
Mucus,	1.4
Alcaline muriate,	1.7
Lactate of Soda and animal matter,	0.9
Pure Soda,	0.2
						<hr/>
						100.0

Doctor Rayer having sent me a little time since a certain quantity of saliva, which came from a lady who secreted nearly two pints of it daily, I availed myself of the opportunity to make some experiments, which I have not been able to follow up as carefully as I could have wished, but which, notwithstanding, appear to me calculated to modify some of the results annouced in the last place by M. Berzelius.

1°—50 grammes of this saliva were evaporated to dryness; the residue weighed 0.28 gram.; which gives:

Water,	994.4
Fixed matter,	5.6
						<hr/>
						1000.0

2°—The saliva diluted in an equal part of water allows but a very imperfect deposition of its mucus; but about the three-fourths of the liquid are easily filtered through a double paper filter; the remaining fourth resembles a thick glair, and will not pass through. This glair, which constitutes the mucus of the saliva, is reduced almost to nothing by desiccation: however, the residue is yellowish, semi-transparent, and a little unctuous, as if it contained a fatty matter. It swells, and is divided in potass, and is probably dissolved in a slight degree in it: but the greatest part remains insoluble in it,

which is contrary to the observation of M. Berzelius, and destroys one of the proofs on which he rests for saying that the phosphate of lime does not exist ready formed in it.

3°—The filtered saliva is as transparent as water. It becomes turbid at the boiling temperature, or by the addition of the deutochloride of mercury, which appears to me to prove that it contains a small quantity of albumen, evaporated to dryness, it left a yellowish residue, which I treated with alcohol at 38°. The alcohol being evaporated presented a little micaceous matter, and an odour, of extract of meat; moreover, it was almost entirely saline, and deliquesced in the air, excepting a salt in isolated crystals, heavy, almost microscopic which must be chloride of sodium. The liquor of deliquescence precipitated copiously the chloride of platina, and potass did not disengage any ammonia from it. On adding water it was all dissolved, even the substance of a micaceous appearance, and the solution presented the following characters:—

Turnsol, no action;

Lime Water, no precipitate;

Nut-gall, a precipitate;

Nitrate of Silver, a very abundant precipitate of chloride;

Oxalate of Ammonia, nothing at first: after some time a well-marked precipitate.

Deutochloride of Mercury, nothing.

4°—The remainder of the liquor was evaporated to dryness, and calcined: the odour like that of burning glue, residue carbonaceous; then a saline residue, white and strongly alkaline, precipitating copiously chloride of platina.

These results indicate in the part of the saliva soluble in alcohol: the presence of chloride of sodium, and no doubt of potassium, some lactate of potassa or soda, since it is agreed to give the name of *lactic acid* to the organic acid, which thus neutralizes the alcalies in a great number of the animal fluids.

A small quantity of a calcareous salt: an animal matter presenting the characters of osmazome. They demonstrate in them the absence of the phosphates and of the ammoniacal salts.

5°—The part of the saliva filtered and dried which had not been dissolved by the alcohol, was treated with water; a membranous residue, and a yellowish, thick, transparent liquor, restoring the blue colour of turnsol reddened by an acid. Thus the saliva contains a salt which re-acts as an alkali; but I do not think that it is of pure soda, as one might suppose from the expression used by Berzelius: for the pure soda would have been dissolved by the alcohol. I do not even think that it can be carbonate of soda, which, as we now know from the experiments of M. Pelouze, is not decomposed by alcohol acidified by means of acetic acid: I conceive this salt to be phosphate of soda, the presence of which will be proved: at least this presence is sufficient to explain the alkaline re-action observed by the aid of the tincture of turnsol.

This liquor even precipitates gall-nuts very copiously, a result contrary to what has been noticed by M. Berzelius. The precipitate is pulverulent, or divided in the liquor, instead of being agglutinated, tenacious, and elastic, as it would be if formed by gelatine.

This liquor is not disturbed by chlorine, a reagent which precipitates gelatine in so striking a manner. It is not precipitated by chloride of mercury.

6°—Neither is this liquor when evaporated to dryness either gelatinous or transparent; the product is dull and opaque, and presents, moreover, a great quantity of crystals which are square, or a little flattened. This product being calcined, at first gives off the odour of gelatine which is burning, then it leaves a saline residue soluble in water, which presents with re-agents the following results:

Turnsol reddened, restored to a blue colour, perhaps more feebly than before calcination:

Nitrate of Baryta, a flaky, copious, precipitate, which is dissolved in nitric acid, with the exception of a whitish cloudiness, owing to a small quantity of sulphate:

Nitrate of Silver, a yellowish precipitate, which becomes white, and diminishes in quantity on the addition of nitric acid:

Oxalate of ammonia, nothing.

These results indicate chloride of sodium, phosphate, and sulphate of soda. I did not ascertain the presence of potass by chloride of platina.

7°—The product of the saliva, which was dissolved neither in alcohol, nor in water, was in the form of white, opaque membranes, consistent and elastic. Potass dissolves but a portion of it. The alkali diluted in water, filtered and neutralized by an acid, is not at all disturbed; but the liquid becomes frothy like saliva diluted in water.

This same residue treated with nitric acid without heat becomes a little yellow, then is entirely dissolved in it by degrees.

Heated in a crucible it evolves the odour of burnt horn, becomes black, then yields a white residue of calcareous phosphate, almost as large as it was itself.

From these trials, which I must acknowledge do not possess all the precision which might be desired, it appears to me to result, that the saliva contains,

1°—Some *mucus*, such as M. Berzelius has defined it, characterized by its insolubility in water, the great quantity of this liquid which it absorbs and retains in its natural state, and the great quantity of calcareous phosphate which it yields by combustion.

2°—A particular animal matter, which accompanies at once the salts soluble in alcohol, and those soluble in water. This substance approaches very near to gelatine by the odour which it yields on burning, and by its precipitation by means of gall-nuts: but it differs from it by this same precipitate which is pulverulent, and not tenacious and elastic, as that produced by gelatine; because it is not precipitated by chlorine; lastly, because it does not form jelly when concentrated.

It is not therefore gelatine. It is not *osmazome*, if by this term we understand a compound extractive possessing smell and taste, which evidently contains a great number of principles foreign to animal matter properly so called; but it is, as I think, the azotized matter of *osmazome*, separated from the salts, and from the odorous principle usually accompanying it; and what proves to me that this odorous principle is not essential to it, is that the portion of this substance, which followed the salts soluble in alcohol, was possessed of the well-known odour of *osmazome*, whilst that which was dissolved in the water, and which differed not from the former, was destitute of it.

3°—Albumen in the state of solution, presenting in the dried saliva several of the characters of mucus, and sensibly yielding like it a considerable portion of phosphate of lime by its decomposition in the fire; but this I attribute to the circumstance of the mucus not being completely insoluble in the saliva; so that this liquid when filtered contains a small quantity of it, besides the albumen and the other animal matter.

4°—Soluble salts, which are chlorides of sodium and of potassium, some lactate of potass and of soda, a salt of lime, some phosphate, and sulphate of soda.—*Journal de Chim. Med.*, April, 1833.

BOTANY AND NATURAL HISTORY.

On the Chemical Transformations effected on the Vegetable Juices under the vital Influence.—At a meeting of the Royal Academie des Sciences, held on the first of April, M. Biot read a paper relative to his new experiments on vegetation, explaining the first results of his researches on the chemical transformations effected on the vegetable juices under the vital influence. According to his experiments the author announced that the sap of the birch-tree, of the walnut-tree, and of the sycamore, tried at this season, at the time it comes from the tree, contains not any sensible quantity of carbonic acid; whence he concludes that the young buds, which are nourished exclusively by this sap, before the development of their external organs, must have the power of decomposing sugar, and, in general, the carbonized products which it contains, in order to appropriate to themselves the carbon, and make it serve for the rapid development of their foliaceous parts, in the same manner as the plumulæ of the grain in germination decompose the fecula of their cotyledons, or of their perisperm, and convert it into sugar, with which they are nourished. Accordingly, as he now conceives, he found that the young buds of the lilac, those only which are already uncovered by their shells, contain sugar, a fermentable sugar, similar, by its direction to the left, to the unsolidified sugar of the grape. Afterwards, he extracted the liquid and soluble elements now contained in the wood of this shrub, and in like manner sugar was found in it, as in the wood

of the walnut-tree and sycamore; but this sugar, which is that of the sap, is analogous, by its direction to the right, to the sugar of the cane or of starch.

Thus the vegetation of the bud has the power of changing these products one into the other, as germination changes the dextrine of the fecula into a sugar turning towards the right, (M. Biot has satisfied himself of it in the case of barley when germinating,) and his results contain nothing that is not conformable to the new properties which we every day see develop themselves in similar chemical actions. For instance; M. Bouchardot announced that the sugar from the cane, when subjected to fermentation, is changed into uncrystallizable sugar, and it appears that M. Dubrunfaut had on his part made the same observation. Now, in observing the direction of the rotation of this product, M. Persoz has ascertained that it is analogous to the unsolidified sugar of the grape, for its rotation takes place towards the left, whilst the sugar of fecula, submitted to fermentation, kept its rotation towards the right, until it was entirely destroyed. This means of distinction, says M. Biot, will be very useful in the study of the vegetable juices: but now, taking it as a mere fact, since fermentation effects such changes, it is clear that vegetation, which is a species of living fermentation, may equally effect them.

At the same sitting, M. Edwards read a paper on the formation of acetic acid by germination. Having undertaken at the commencement of last spring, with M. Colin, Professor of Chemistry at the School of St. Cyr, a series of researches to determine the influence of physical and chemical agents on vegetation, this physiologist has left aside the influence of electricity, with which he knew that M. Becquerel was engaged. I was not present, says M. Edwards, at the last sitting where M. Becquerel commenced the reading of a very interesting paper on germination. I merely learned that he was there to detail some researches on the presence of an acid which is developed by germination, and that this acid is acetic acid. I knew merely this fact, and it is precisely this upon which we are agreed, without there having been any previous communication between us on the subject, notwithstanding the intimacy and friendship which subsist between us. On the part of M. Colin and myself this fact has been verified in a long series of experiments on a variety of grains, and we have also observed the continuance of it a long time after the escape of the radicle and the cauliculus, at least as long as the cotyledons continue to perform any action.

With respect to the development of this acid by the action of the leaves, a development proved by M. Becquerel, we have not observed it, and indeed we scarcely could do so, for that did not come at the time within the circle of our researches.

We delayed making this communication, because we proposed to ourselves to present a series of researches in a methodical order, and each matter was to be mentioned in its own place: thus at present I confine myself to announcing the fact, proposing to return to the subject at a subsequent period. I shall only add, that the fact is impor-

tant, as may be anticipated, and as we shall afterwards shew, when we shall make known also other products manifested out of the grain under certain circumstances, during the act of germination, such as those of alcoholic fermentation, or, when life ceases in the grain, the formation of a product with properties opposed to acidity, that is to say, an alcali.—*Archiv. Gen.*, April, 1833.

ANATOMY AND PHYSIOLOGY.

Biographic Notices of Scarpa and Paletta.—Antonio Scarpa, born in 1748, in the village of La Motte, in Friuli, studied in the University of Padua, where the celebrated Morgagni was his first instructor. Endowed with an extraordinary talent for Anatomy, Scarpa soon acquired precise and extensive knowledge in the science of organization, and, at the age of 22, he filled the chair of Anatomy in the University of Modena: it is there he composed his first work, the Result of his Researches on the Structure of the internal Ear. The moments of leisure which he borrowed from his public instructions, and from his attendance at the Military Hospital, of which the Grand Duke had appointed him head Surgeon, he dedicated to anatomical pursuits; in 1779 he published his Observations on the Structure of Ganglions, and of the nervous Plexuses. A short time after he undertook a journey to France and London, which gave rise to his correspondence with Vicq. d'Azyr, Pott, and J. Hunter. It was to his meeting with Brambilla at Paris, that he was indebted for the honourable offers made to him by the Emperor Joseph 2d, of the chair of Anatomy in the University of Pavia. Scarpa, influenced solely by the sentiments of gratitude which he considered he owed to the Duke of Modena, did not accept it till invited by the Duke himself. His installation took place in 1783, and he delivered on that occasion a remarkable discourse with regard to the means of perfecting the study of Anatomy. From this period to the end of the 18th century, are dated all the other researches of Scarpa on the Anatomy and Physiology of the Organs of Hearing and of Smell, on the Nerves of the Heart, on the Structure of Bone, &c. &c.

In consequence of these valuable works Scarpa was considered as one of the first anatomists of the 18th century; from the commencement of the 19th, his writings and practical observations already pointed him out as one of the most celebrated surgeons of our time. Thus he himself became an example of that truth which he had expressed in his eulogium on Carcano Leone, when he said: "The history of surgery furnishes us with a useful lesson, by shewing that those surgeons whose names have become celebrated, have always previously had the characters of being excellent anatomists." He marked his entry on this new career by the publication of his Treatise on Diseases of the Eyes, a distinguished work, which was translated

into all the languages of Europe, and which became classical at once in Italy, France, and England. A short time after appeared his *Essay on Club-feet*, which may be justly considered as a model of the application of mechanical means in the treatment of this deformity. The Society of Medicine of Paris had proposed for the year 1798, the solution of the most important questions of the anatomical and surgical history of aneurism. Scarpa, already rich in numerous observations on this subject, undertook the solution; but time not having allowed him to finish his work quickly enough, he did not complete and publish it for several years after: still the Society of Medicine of Paris may be justly proud of having excited him to the undertaking. This work, and that which he published at a subsequent period on *Hernia*, establish the most splendid claims for Scarpa's glory. It is there this celebrated author has evinced all the superiority of his talent as an observer and a practitioner. The precepts which he has laid down will hold their place in science; every day experience justifies their exactness.

In 1812 Scarpa ceased to devote himself to the business of instruction; he was then named Professor Emeritus of Anatomy, of Clinical Medicine, and of Surgical Operations, and in 1814, Director of the Faculty of medicine of Pavia. The plan of studies which had been marked out for this school, did not meet the exigencies of science; it reached not the height of the present state of knowledge, and the execution of it was in many respects impracticable. Scarpa submitted to the government reflections full of wisdom and of energy, in order that the requisite modifications may be introduced into the university regulations. His repeated observations remaining unanswered he gives in his resignation, which was accepted; but the authorities appointed no person in his place. From this time Scarpa, though retired from active life, ceased not to prosecute the cultivation of a science in which he had made so great a progress. It is since this period that he published his papers on the lateral Operation with the Gorget of Hawkins, which he had modified, on the high Operation, and on the recto-vesical Operation, against which he exerted himself with considerable ardour, and which was the subject of his controversy with Vacca Berlinghieri, on Scirrhus and Cancer, on Hydrocele of the Spermatic Cord, on Ascites complicated with Pregnancy, and on the manner of performing Paracentesis under such circumstances. He has also described, under the name of aneurism by anastomosis, an aneurismal dilatation of the arteries of the bones, which is always accompanied with destruction of the osseous tissue. He has also written on cataracts, artificial pupil, hernia of the perineum, and on several other subjects, which he published in the latter years of his life.

When we compare the number and importance of the practical results for which we are indebted to the works of Scarpa, with the limited extent of the theatre of his observations, one cannot help feeling amazement at his having been able to perform so much with so few resources. Why was it not given him to explore one of those more vast plains where facts rise without number, and which, how

ever, continue for so long a time barren with respect to science? So placed, Scarpa, who retained to the end all the energy and activity of youth, and who was so thoroughly penetrated with the love of his profession, would have increased its domain still more.

To skill as an anatomist, Scarpa united a distinguished talent for drawing; the splendid plates which accompany all his works serve considerably to assist his demonstrations. He possessed a clear, animated voice; delivered his ideas with remarkable order and method, his language being at once clear and elegant, whilst his enchanting eloquence captivated the entire attention of his numerous hearers. He was of the middle size: his manner was tinged with a degree of austerity, tempered, however, by amiability and remarkable sweetness. Being passionately fond of the fine arts, and particularly painting, he amassed at considerable expense a magnificent collection of original pictures from all the Italian schools; he wrote also several letters on this subject, and on different subjects of antiquity.

Scarpa died the 30th of October, 1832, at the age of 84 years, after several years of suffering from a chronic inflammation of the bladder, and a calculous nephritis. Napoleon, who knew well how to honour real merit, created Scarpa Knight of the Iron Crown, and subsequently Member of the Legion of Honour.

The French Institute counted him among the number of its foreign fellows. He was also member of most of the learned societies of Europe.

PALETTA.—Jean Baptiste Paletta was born in the year 1747, at Monte-Crestese, a village in the valley of Ossola, in the Sardinian States. He commenced his studies at Briga, and afterwards came to Milan, to attend the courses of anatomy, medicine, and surgery, under Patrini, Gallardi, and Moscati: his rapid improvement and persevering zeal soon procured him the place of resident pupil (*élève pensionné*) in the great hospital of Milan. He was already possessed of profound surgical knowledge, when he repaired to Pavia to attend the lectures of the celebrated Morgagni, and to take in that University the degree of Doctor in Medicine. From this period the reputation of Paletta commenced, and Maria Theresa then wished to appoint him to the chair of Anatomy in the University which she intended to found at Mantua; but his love for his country made him refuse this honourable situation, and he returned to Milan in 1774: he was then 26 years old. From this time he devoted himself with extraordinary zeal to the study of pathological anatomy and of surgery; and in 1778, he went to obtain the degree of Doctor in Surgery at the University of Pavia. On his return to Milan, Paletta filled there successively the place of Joint Surgeon, Surgeon in Ordinary, Demonstrator of Anatomy, and Professor of Clinical Surgery: finally, in 1787, he was appointed Head Surgeon to the great hospital of Milan, where the lectures, as well as the character of the Professor, shed a brilliant lustre on the University.

The writings of Paletta are as remarkable for their erudition, as for the talent for observation possessed by their author. In all these

works he displays at once the profound anatomist and the skilful practitioner : the merits too of his works, justly appreciated by the learned of all countries, gave to Paletta's name an imposing authority in science. The Medico-Chirurgical Academy of Vienna, the National Institute of the Sciences and Arts of Milan, the Society of Medicine of Bologne, of Lucca, Venice, Modena, Naples, &c., counted Paletta in the number of their most distinguished members : he was Knight of the Iron Crown, and Member of the Legion of Honor. He died the 27th of August, 1832, at the age of 85. He wrote several works.—*Archiv. Gen. de Med.*, March, 1833.

Memoir on the Functions of the Encephalon, including a brief Notice of most of the recent Doctrines.—M. Rolando considers that his experiments have proved that the medulla oblongata is the centre of sensibility, the focus and source of life. Treviranus regards it as the centre of animal life, at least. Fodera deems it to be the “hypomoclion” or under lever of life. It is, according to him, the main exciter of respiration, circulation, digestion, and of voluntary motion ; the influence which it exerts, proceeding in an especial degree from the point whence the pneumo-gastric nerves arise ; this point is the line of demarcation between the medulla oblongata and spinalis. The portion of the medulla situated below this point serves only to transmit impressions received ; and the portion of the medulla oblongata situated above this point, is not the source of any influence, nor the centre of any action. The brain and cerebellum are only appendages to the medulla oblongata ; and all irregularities of the sentient and moving powers, which are observed in diseased states of the mass of the encephalon are referable to sympathetic derangements in the functions of the medulla oblongata ; for this latter part is invariably the only real and true seat of palsy. According to M. Serres, the whole encephalon is endowed with sensibility, but the medulla oblongata is the principal seat of it. M. Hourens places the source or centre of vitality, and of the nervous power, in the medulla oblongata. All other parts of the nervous system, he says, are dependant on it. If the medulla be divided below the point at which the pneumogastric nerves arise, it dies, and the encephalic mass remains alive ; if the section be made above this point, the very reverse ensues. Hence this point is termed by him the vital knot, the central bond of the nervous system, the base of the nervous tree, the encephalon representing the trunk, and the spinal cord and nerves, the roots of it. Respiration altogether depends primarily upon it ; and it is not less indispensable for voluntary motion.

To none of these doctrines do we subscribe ; the source and centre of life do not reside in, nor depend upon, any special organ, or set of organs, but each part is endued with vitality, and this vitality is a result of its organization. Neither animal nor organic life has an exclusive centre of influence or acting power ; and, therefore, sensibility in its widest acceptation has no special, or particular seat. Every system, nay, every part of any and more especially of the nervous

system, has in fact an independent existence or vitality, and hence we cannot admit to any part exclusively what some authors have called "primordially of action." Before we proceed further in our account of the functions of the medulla oblongata, let us determine what is the precise portion of the nervous system to which that term is to be applied. The older anatomists, and the most celebrated modern ones, include not only the rachidian bulb, or tail of the medulla oblongata, but also, the tuber annulare, and the crura cerebri et cerebelli; but we object to this classification, and while we are inclined to restrict the appellation to the rachidian bulb, and the ganglions which it comprehends, viz. the pæumo-gastric, olivary or vocal, the acoustic, or auditory, and the gustatory or trifacial ganglions, we annex to them the optic and olfactory bodies or lobules, which have usually been considered as belonging to the cerebrum. The reasons of our disjoining the tuber and the crura cerebri and cerebelli are that their organization, their mode of development, and their relative bulk, as well as the results of experiments upon them, do not agree with the structure, growth, or functions of the medulla; whereas, in these very particulars, there is a striking resemblance between the optic and olfactory lobules, on the one hand, and the rachidian bulb on the other; and we have therefore associated them together. The medulla oblongata, as thus defined, is, as it were, superadded to the spinal marrow, and the functions of the one are analogous to, but are more complicated and more perfect than those of the other; and again it is subordinate, alike in organization, position, and complexity of function, to the other parts of the encephalic mass. The medulla oblongata is to the organs of special sense what the spinal cord is to the organs of general sensation and locomotion, (the skin and ordinary muscular apparatus.) The former, viz. the organs of the four special senses are provided not only with ganglionic nerves (for the purposes of organic or nutritive life) and nerves of general sensation and of general muscular motion, but also with two sets of particular or super-added nerves; the one being destined to transmit peculiar impressions from the organ to the nervous centre; the other, to receive the motive or existative influence from this centre, and to convey it to the organ. Now with these nerves of special sensation are associated certain peculiar or proper motor nerves; thus the hypoglossal is associated with the lingual, the third and fourth pairs, the ophthalmic of Willis, and the 6th pair, with the optic; and the facial with the auditory. The sense of smell does not require any general movement, and its muscular apparatus or appendages are imperfect; it is therefore not provided with a special or proper motor nerve, but is supplied with twigs from the facial and trifacial nerves. The relation which we have thus pointed out to exist between the auditory and facial nerves, co-operating simultaneously for the same function, although in a different mode, and the consideration of the mutual relative development of these two nerves, shew us the error of those who have regarded them as quite unconnected with, and independent of each other. In the same manner, the pæumo-gastric nerve, on

the one hand, and the spinal on the other, are in reality only two parts of the same nervous apparatus; the one consisting of sensitive filaments, and the other of motive filaments; co-operating with this apparatus, are other cords of filaments sensitive as well as motor, which proceed from different parts; these cords are the glosso-pharyngeal, the hypoglossal, the phrenic, and the intercostals. The first phenomena, or set of actions, which follow any impression made on an organ of sense, and transmitted along its nerve to its respective ganglion, whether of the medulla oblongata, or spinalis, take place in the locomotive, or muscular apparatus with which the organ affected is more especially associated, certain movements occur, and for the purpose either of defending it from the too powerful impression of the external agent, or of better accommodating and arranging it, for such an impression; thus in the case of the eye, or of the ear, the first movements which follow a noise, or strong light, are certain automatic and involuntary actions of the muscles subservient to the organs of vision, or of hearing: the object of which actions is either to mitigate, or to increase and concentrate the external impressions. Now these actions are under the influence solely of the medulla oblongata; but the perception of the impressions belongs to the brain, and to the brain alone; for the brain is the organ of the faculty which wills, and of the faculty which perceives and judges. We are therefore not to regard the medulla oblongata and spinalis as the seat of special and general sensation; they are, so to speak, points of transition, or organs intermediate between the external part on which the impression is made, and the brain which perceives the impression; the function of the medulla being rather to excite a train of movements (those of general locomotion), destined to withdraw the individual organ, or to approximate it to the external agent, or, at other times, to modify and alter the condition of the organ itself; in short, to determine and regulate those special actions which are necessary to the accomplishment of the functions of sense.

MM. Majendie, Dumeril, and Serres, suppose that the trigeminal nerve is fundamentally the real nerve of all the special senses; that it bestows, by a mode of influence which we do not understand, the special sensibility on the optic, olfactory, auditory, and gustatory nerves; and that frequently, especially in the lower animals, it takes the place, and exercises the functions of these nerves. The following are the chief arguments adduced in favour of these opinions. 1. In the mole, there is no optic nerve, but instead of it, there is a twig of the fifth pair; in fishes, there is no auditory nerve; in the cetacea, no olfactory nerve; and yet these animals have in all probability a certain degree of the corresponding sensations. 2. When the fifth pair is divided, all the special senses are destroyed, or at least enfeebled; when the special nerves alone are divided, the animal still retains in part the capability of perceiving odours, tastes, sounds, and light; but if the fifth pair is divided at the same time, these sensations are utterly and entirely annihilated. 3. Certain of the special sensations may be produced in other ways than the contact or impres-

sion of their ordinary stimuli; thus, for example, sound may be heard although the external ear be plugged up, if a watch be applied to the head, or put between the teeth. 4. The occurrence of amaurosis, anosmia, and ageusia, from neuralgia of the trigeminal nerve. In answer to these arguments, we may state that division of the fifth pair does not immediately and instantly destroy any of the special senses; that a certain interval elapses, and during this interval, certain changes have taken place in the texture of the parts on which the nerve was distributed; thus in regard to the eye, the cornea becomes opaque, the conjunctiva inflamed, and the humours muddy; the same results follow any injury or disease affecting this nerve: very different, however, is the effect of the division of the optic nerve; the sight is gone instantaneously and for ever. As to hearing the ticking of a watch, when put between the teeth, it is easily explicable on the well-known laws which regulate the transmission of sonorous vibrations; in a similar manner can we understand that those animals which have no external ear, as seals, moles, fishes, &c., may yet have the sense of hearing.—And again, can we not hear the noise of our own voices, even when the ears are firmly plugged, and excluded from any atmospheric impulsions on the tympanum? We readily admit, that the 5th pair is intimately connected with the functions of the four special nerves; but this connexion is indirect, and arises from the trigeminal being the nerve of general sensibility, and of common mobility to the hand and face; and when it is destroyed, or disused, certain morbid changes take place in the organization of the structures which are dependent upon it. But let not our readers be deceived, and suppose that these organic changes which ensue, are a proof that the trigeminal nerve has any direct influence upon the circulation, absorption, exhalation, and secretions of the organs, either in a state of health, or of disease: all these functions, and all morbid states of them, such as inflammation, ulceration, oedema, &c. are dependent upon, and regulated by, the great sympathetic nerve, which we know to be connected by several twigs with the fifth pair. The only direct result of the injury of the fifth pair, is the loss of common sensibility in the exterior surface of the eye and face, palsy of the muscles of the face and tongue, and an immobility of the eye. It is curious that any lesion of the great sympathetic, even in the neck, is followed by inflammation, dulness, and atrophy of the eyes; in short, by all the consequences of an injury of the fifth pair itself.

Tiedemann observes that the great sympathetic, by the influence which it has on the general phenomenon of nutrition, and therefore on the organic condition of parts, and on the functions of these parts, such as the secretion of the humours of the eye, and of the mucus of the nose, must necessarily exert a great control over the phenomena of sensation, more especially of smelling and of sight. We have said above, that Majendie and others assert that the mole has no optic nerve; Carus and Geoffroy St. Hilaire, however, are of a different opinion, and state that this animal has at least the orbitary portion of the nerve, that it is of the size of a hair, and is united to the ophthal-

mic branch of the fifth pair, to form with it the retina. This is still a disputed question; and although Hilaire supposes that the functions of any of the sensorial nerves are never transposed to, nor performed vicariously by, another nerve or nerves, yet we cannot deny, that in the invertebral animals, and even in some families of the vertebral ones, the divisions of the trigeminal nerve appear to supply the place and perform the duties of the proper nerves of sense. In those animals which have no olfactory or gustatory nerve, or in which they have been cut, the probability is, that the sensation which belong to these special nerves, are reduced to the phenomena of ordinary and general feeling, or sensibility. There may be, quite possibly, a certain positive action of light on the minute and pulpy twigs of the fifth pair in the eye: for light must be regarded as a species of matter, and must therefore make an impression, however weak, and almost unappreciable, on a sentient part; but this impression is only a modification of the general phenomena of touch. The transformation, therefore, of nerves of general sensibility, into nerves of special sensation, is to be regarded as erroneous and unsatisfactory. Bell and Shaw have pointed out the many fallacies into which Majendie has been led, by confounding the phenomena of special, with those of common sensibility; and Eschricht has lately published a suite of experiments which are at utter variance with the conclusions of the French physiologist.—*Medico-Chirurgical Review*, April, 1833.

PATHOLOGY AND THERAPEUTICS.

Emphysematous Tumour in the Neck, by Dr. Ollivier.—M. R——, of a sanguineous and nervous temperament, and robust constitution, in the habitual enjoyment of good health, came one morning to the house of one of his friends to talk over some matters; he sat down on a chair, and turning back his head to rest it on the back of the chair, he felt all on a sudden, low down in the neck, a sort of crackling noise, accompanied with slight pain, and at the same instant a tumour, the size of two fists, developed itself there, causing inexpressible anxiety, uneasiness in the region of the heart, and partial loss of consciousness. They pressed round him, considering him as attacked with an apoplectic stroke, and wished to send for a physician to bleed him; but M. R. came to himself, the tumour seemed a little diminished, he merely took some sugar and water, and perceiving that refraining altogether from motion lessened his uneasiness, and that the tumour in the neck sensibly decreased in size, he remained tranquil, without changing his position, for nearly two hours, and when he found himself better, he ascended a carriage and returned to his own house.

Being called in subsequently by the patient, I was unable to visit him before 7 o'clock in the evening. Instantly after his return home,

he had been put to bed, and the shivering, with which he had been at first seized, was succeeded by a general copious perspiration, which he had brought on and kept up by a great quantity of covering. Thus I found the patient with his face red, moistened with sweat, the head stupid, the pulse full, 88 pulsations in the minute. After he had told me what I am just after stating, I carefully examined the region of the neck, where the tumour appeared. The place he pointed to me was the anterior and inferior part of the neck, within the sterno-mastoid, and directly behind the right sterno-clavicular articulation.—There was no tumefaction there, but this part of the neck was manifestly more swelled and softer than that of the opposite side: on pressing gently over the sternal extremity of the clavicle in the direction of the trachea, there was caused deep-seated pain. Deglutition was not at all interfered with, and gave no pain: neither did the different movements of the head and neck produce it: the patient complained merely of an acute and circumscribed pain in the substance and centre of the right breast: a troublesome sense of weight in the epigastrium, and of a difficulty of breathing on the right side: he was moreover very restless.

I succeeded in tranquillizing him, and when I saw him become more calm, I prevailed on him to tell me more in detail what he experienced. M. R. recommenced his narrative, and spoke to me with considerable animation for some minutes, when all of a sudden I saw him become pale, his voice became more weak, and in an instant the skin of the neck was raised in the part indicated by a tumour the size of a large orange: at the same time the patient made repeated efforts to vomit: a cold perspiration succeeded to the heat which existed previously: the respiration became difficult, intermittent, accompanied with a lacerating pain on a level with and inside the right nipple, which corresponded with a painful dragging felt in the inferior part of the neck: the patient remained in a sitting posture, the body bent forwards, &c., complains every instant that he feels himself dying, and in spite of his extreme paleness, and alarming syncope, I do not find the pulse more irregular, or smaller, I reckon even the same number of pulsations as before.

This state of anguish lasted for several minutes during which the aspect of the patient had something frightful in it. The tumour was enlarged, and gave to the neck the form which it presents in certain large goitres, which engage but half the thyroid gland: the skin covering it was of the natural colour. The rapid development of this tumour, its external characters, its seat, which explained to me most of the phenomena which I had before me, all announced to me that it arose from a circumscribed emphysema which probably had its origin in a very small rupture of some point of the trachea. The terror of the patient, more than the sensibility of the tumour, prevented me from satisfying myself whether there existed the crepitation usually found in emphysema; I doubt not but I would have discovered it, had I been able to make sufficient pressure on its surface.

In consequence of this diagnosis, I recommended to the patient

absolute rest and silence, in order to avoid every species of exertion which might for a moment accelerate the respiration: I prescribed some narcotic medicine, with cold aromatic frictions, and a compressing bandage on the tumour. I endeavoured above all to encourage the patient's mind, whom this unexpected attack had very much alarmed, by explaining to him the nature of what had happened to him. The night was spent in a very restless and sleepless state: the frictions were repeated at an early hour, and when I visited the patient on the following morning, the tumour in the neck had almost entirely disappeared, but an annoying sensation of weight in the epigastrium, and pain in the parietes of the chest, were constantly felt, though in a less degree. These symptoms decreased progressively the following days, the pain in the lower part of the neck became more and more obscure, and after a week's confinement and rest, M. R. was able to resume his ordinary occupations. For the last four months his health has not experienced the least derangement.

Examples are met with in authors of spontaneous emphysemas being caused in consequence of violent efforts of respiration: but here there was no such cause; yet the emphysema develops itself then, precisely in the same way as in the case which I am after stating, around the clavicles, and most frequently this occurrence is not attended by fatal consequences. In my patient, the particular symptoms which he presented incline me to think, that the air escaped in small quantity, probably through a laceration in the side of the trachea: that it made its way into the cellular tissue surrounding the trunk of the pneumo-gastric nerve, whence result a displacement, and a sort of distension of this nerve, to which may be attributed the spasmodic phenomena, of which the stomach was the seat, as well as the disturbance of the respiration: in fact these phenomena were in the direct ratio, with respect to their intensity, with the volume of the tumor in the neck, so that they increased according as its volume was augmented, their diminution followed the subsidence of the emphysematous parietes.—*Archiv. Gen.*, Mars, 1833.

Hydatid (enkysted) at the base of the Cranium: Atrophy of half the Tongue, and Paralysis of the left half of the Pharynx and Larynx.—M. Boulland read a paper of M. Montault, entitled: *Remarkable Case of Pathological Physiology of the Nervous System.* A person of the name of Girard, aged 33 years, having worked for a considerable time in low and damp places, after having fallen in 1828 on the posterior part of the neck from a very high stairs into a cellar, where he worked at his trade as a weaver, began to feel in the left posterior and lateral part of the head, very acute pains, which soon extended to the corresponding side of the neck. These pains caused want of sleep, a sense of tightness, and dragging in the muscles of this region. There was afterwards added to this considerable difficulty in speaking, so that at certain times he could scarcely make himself understood. After several fluctuations in the state of the patient, the pains became so violent, that towards the month of Sep-

tember, 1831, the motions of the head on the neck became altogether impossible. He was admitted into the Hotel Dieu, under the care of M. Dupuytren. His situation then was as follows :

The motions of the head on the neck were in part performed by the entire of the cervical column : the tongue was diminished in size, atrophied only on the left side, a circumstance which Girard said that he perceived from the very commencement of his disease ; this atrophy was more developed at its point and in its central portion than at its base : the right side of this organ, on the contrary, appeared better nourished, and to have acquired more strength. Reduced almost to the thickness of the two mucous layers which enclose the muscles, the left side was drawn to the right by the muscles of that side, every time the tongue was brought out of the mouth, whether by reason of the increase of strength of the right side, or because the latter was no longer sustained by the left side. The articulation of sounds was at this time clear and distinct.

The successive application of four different substances, (sugar, sulphate of quina, an acid, muriate of soda,) dissolved in a small quantity of water, afforded an opportunity of ascertaining the changes which might have come in the sense of taste, and the result of the experiment was, that *tastes were perceptible by the left side of the tongue*, (the side atrophied.) M. Dupuytren was inclined, in consequence, to conclude, that of the three nerves which are distributed to the tongue, (the hypo-glossal, glosso-pharyngeal, and lingual,) it was to the alteration of that of the ninth pair, or of the hypoglossal, that the atrophy of the tongue must be referred. Further, considering that the intellectual functions, and the motion of the limbs, were not at all affected, M. Dupuytren thought that the lesion of the nerve existed at its exit from the cranium, and not at its origin. All the means employed producing but little improvement, the patient left, re-entered, left, and again returned to the Hotel Dieu, the 15th of December, 1831. He was then placed under the care of M. Gendrin. He was now worse than ever. M. Montault having repeated the above-mentioned experiments, obtained the same results as M. Dupuytren. Numerous means were used to no purpose. Tired out, as he said, at seeing no cure, he once more left the Hotel Dieu, the 24th of March, 1832. On the 28th of the September following, Girard returned to the Hotel Dieu, and was placed in the wards of M. Rostan. He at that time felt a constant pain, which extended from the great occipital foramen to the third or fourth cervical vertebra ; a small tumour also was discovered behind the mastoid process. Deglutition began to become difficult, to such a degree, that every time the patient drank, a small quantity of the liquid always fell into the larynx, as was made evident by the cough and pain. The diagnosis of M. Rostan was, *fungous tumor of the dura mater*. Hiccup, vomiting, obstinate constipation, fever towards evening, unfavorable presentiments, such were the leading symptoms observed during the month of October. The 6th November the patient went out. The 20th December following, he entered the Hospital Cochin, under

the care of M. Gendrin. At this period he continued quite motionless in his bed, in order, as he said, with a voice now nearly extinct, not to increase the pain of the neck: the general sensibility was diminished over all the left side of the body. The loss of voice increased more and more, as also the power of swallowing. This unfortunate individual was obliged to spend entire hours in swallowing a spoonful of soup, the only nourishment he liked. The hiccup became almost constant; some attacks like those of epilepsy developed themselves, (the heart at no time presented any thing remarkable.) At last, on the 12th of January, 1833, after having swallowed some spoonfuls of soup, with the difficulty above alluded to, Girard let his head fall back on his pillow; he was dead.

Post mortem.—Nothing remarkable in the thickness of the parts, either on the surface of the cranium, of the membranes, or of the brain; except that the cerebral substance is firmer than ordinary, and the ventricles are dilated by a very copious transparent serum. On raising the left half of the cerebellum, and in the left lateral fossa of the occipital bone, there is a kyst, the size of a hen's egg, which contained serum and a number of hydatids. It was not adherent to the surrounding membranes, and appeared at first to float freely in the cavity of the arachnoid. After having penetrated to the depth of some lines into the spinal canal, the kyst gave out a sort of appendix, sinking into the anterior condyloid foramen, and contained a hydatid which seemed to strive to overcome the resistance of this narrow passage. From the base of the kyst there was detached a second prolongation, which engaged in the anterior portion of the foramen lacerum posterius on the left side, after traversing this opening and passing behind the fasciculus of muscles, known by the name of the *anatomical bouquet of Riolan*, spread itself out as far as beneath the superior extremity of the complexus and sterno-mastoid muscles. (It was this which gave rise to the tumour observed during the continuance of the patient under the care of M. Rostan.) The two kysts, the one without and the other within the cranium, communicated with each other by a sort of neck, or contracted portion, corresponding with the foramen lacerum posterius.

The lingual nerve was perfectly sound on both sides. From their origin to their passage into the foramen lacerum posterius, the glosso-pharyngeal and pneumogastric nerves, as also the spinalis of the left side differed in no respect from those of the opposite side: it was the same with the hypoglossal, or that of the ninth pair, up to its passage into the anterior condyloid foramen. But immediately after the exit of these nerves, a striking difference existed between some of them: thus the left hypoglossal nerve was atrophied, (being two-thirds less than that of the right side,) an atrophy which extended as far as its divisions in the muscles of the tongue, and seemed to have been produced by the pressure made by the small appendix, which the internal kyst sent into the anterior condyloid foramen. The glosso-pharyngeal, pneumo-gastric nerves, as also the spinal nerve of the left side, had likewise been pressed by the portion of the kyst which was prolonged

into the foramen lacerum posterius: however, of these nerves the glosso-pharyngeal alone was one-third less than that of the opposite side. The circumference of the foramen lacerum posterius, the bony tongue which divides it, presented an appearance similar to that which aneurismal kysts produce on bones with which they are in contact. On the left, the muscles, as well internal as external, of the tongue were softened, wasted, with fibres thinly spread, yellowish and flabby, as also those of the corresponding pillar of the velum palati. The œsophagus and pharynx did not appear at all altered in structure: only the latter was reduced to the size of the little finger.

The ventricles of the larynx were entirely filled with a creamy whitish matter, evidently the soup which the patient had endeavoured to swallow some moments before death. (This substance, of which there was a certain quantity in the trachea and bronchi, was the cause of his sudden death.)

The left corda vocalis was wasted away. The vertebræ presented no deformity nor alteration; neither did the spinal marrow, nor its membranes.

M. Boullaud thinks with the author, that the extreme difficulty of pronouncing was owing to atrophy of the glossopharyngeal nerve, a nerve which Ch. Bell has ranked among the respiratory nerves: that the paralysis and atrophy of the left side of the tongue, with preservation of the sense of taste, are perfectly explained by the integrity of the lingual nerve, (a branch of the trigeminus,) in which the function of taste appears to reside, and partly by the atrophy of the hypoglossal nerve.

With respect to the lesion of the general sensibility, M. Montault conceives it may be attributed to the compression caused by the cyst within the cranium, either on the upper part of the spinal marrow, particularly behind, or rather on the mesocephalon.

The compression and consequent diminution of the size of the glossopharyngeal nerve accounts for the paralysis of the organs of deglutition. The hiccups, aphonia, entrance of the aliment into the air passages, and death; all this, says M. Montault, acknowledges as its cause, as also the atrophy of the corda vocalis, and that of the pillar of the velum palati of the left side, the compression of the pneumo-gastric nerve in the foramen lacerum posterius. M. Montault has cited two new facts to prove this assertion still more satisfactorily, to wit, that paralysis of the glottis may present a very great, or even an invincible obstacle to the act of deglutition.—*Ibid.*

SURGERY.

New Apparatus for permanent Extension of the lower Extremity.—Notwithstanding the inconveniences attached hitherto to the different methods of permanent extension, there frequently occur in practice cases of fracture where the surgeon feels the necessity of

recurring to this mode of treatment, whatever objections may in other respects be connected with it. It is not only in cases of fracture of the neck of the thigh bone that the importance of continued extension of the femur is acknowledged; fractures of this bone more or less oblique, and occurring in muscular persons, are so often attended with shortening, when the ordinary apparatus for fractures of the thigh bone is employed, that it is absolutely necessary, in order to obtain an exact reduction, and a consolidation without deformity, to adopt in these cases a method of permanent extension. The object then is to select from all, that which is attended with least inconvenience.

The semiflexed position of the limb on a double inclined plane, used in fracture of the neck of the thigh bone, proposed and used with striking advantage by Sir Astley Cooper, Richerand, Dupuytren, may be employed in fracture of the body of the thigh bone, and is no doubt one of the methods least fatiguing for the patient; but if the double inclined plane is of wood, as Cooper, Sauter, and Mayor recommend, be it fixed, or suspended, I hardly think that the compression of the posterior part of the limb on this plane, though it be stuffed, will not eventually become painful to be supported, and if it be composed of pillows, and not of boards, as Richerand and Dupuytren advise, it is more or less apt to sink, which instantly produces a certain degree of shortening, which cannot be removed but by reconstructing the apparatus, and hence comes frequent motion of the fragments. Besides, here the permanent extension is confided to the weight of the foot and of the leg, sometimes, to be sure, assisted by a cloth folded as a cravat, passing over the instep, and it is necessary that this extension should be in all cases necessary to balance the counter-extension made by the weight of the pelvis. However, the unquestionable advantage of this method is to let the muscles which pass over the situation of the fracture in a state of relaxation more or less complete, since the leg is bent on the thigh, and that again on the pelvis.

However, if there existed a method of permanent extension, the limb being extended and placed flat on the bed, though this method be really efficacious, without presenting the ordinary inconveniences of forced extensions; if at the same time the apparatus were so simple, that we might always have at hand the proper means to apply, I am convinced that it would soon be preferred by all practitioners in most of the cases where extension is become necessary. Such a method I think I have found: I applied it in a case of oblique fracture of the body of the thigh bone, and obtained complete success, without the least shortening. Some days since, a new instance of fracture of the thigh bone badly reduced, and imperfectly consolidated, furnished me with an opportunity of again trying my method. The patient having quitted the hospital, where he was first treated, got another fall: the recent callus was torn, and then he entered the Necker Hospital, and was committed to my care: on his arrival there was a shortening of two inches: the apparatus was applied some days since, the patient

supports it without pain, and the extension is so exact, that there is no shortening; I have no doubt of the most complete success.

One of the conditions of every apparatus of permanent extension for fracture of the thigh bone is, that the counter-extension and the extension be made in a direction parallel to the limb. For if the counter-extension be oblique, it will be made, as in the apparatus of Desault and of Bayer, on the groin and ischium, or rather on the upper part of the inner muscles of the thigh. Now, in this method, what should be desired for the patient is, that the counter-extension be not made, (as often occurs, it is true, in the process of Desault, his oblique band is easily relaxed,) since if it be effectual it excoriates the skin covering the attachment of the inner muscles of the thigh, and gives considerable pain. Neither is the extension, in Desault's process, parallel to the limb: it carries the foot out; it is, moreover, applied over the tendo-Achillis, and the instep, which it bruises. M. Bayer has no doubt made a most important modification in it: the traction is made parallel to the limb, and his machine has great power: but it is a machine, and, in many circumstances, it will be difficult to have it at hand, since it is not to be had even in all the hospitals of Paris: besides, there may be at the same time in the same hospital, several patients whose fracture may require the apparatus for permanent extension: so that it will be necessary to have several such machines. Practitioners know full well that these are real difficulties: besides, the apparatus of this distinguished surgeon, as well as that of Desault's, is chargeable with the obliquity of its counter-extension.

With two splints of Desault's, the external one longer, and the internal shorter, with two long bands, a long ribbon which serves for ordinary purposes, a truss (*un bandage de corps*) laced, or buckled, and the apparatus for fractures of the thigh, we may produce the most simple permanent extension, one also which is most effectual, and easiest to bear. The extension and counter-extension will be entirely parallel to the limb, and both made at the extremities of the internal and external splints.

I commence by applying a roller over the foot and leg up to the knee; this done, I take a ribbon several ells long: the middle part of it I apply to the sole of the foot; the two extremities are directed in and out on the internal and external side of the leg, and brought to a level with the upper part of the roller already applied: if the first band is not run out, I fasten on the leg the two folds of the ribbon, carrying a second roller, from above downwards as far as the ankles: a second band serves me to apply this bandage if the first one is not sufficiently long: the ends of the ribbon are carried from above down, and parallel to the limb, and are again fixed by trusses. It is on these ends which go beyond the sole of the foot 15 or 20 inches, that the extension will be made: we see that on one side, the middle part of the ribbon applied over the sole of the foot, has a fixed point of support, and that on the other its two folds are kept on the leg with all the force of a double roller, without the skin being at all excoriated

by their friction, since the first roller separates it from the ribbon. I cause extension to be made during the reduction of the fracture on the lower part of the leg, in order not needlessly to drag the rollers; I treat the fracture in the usual way: then to make counter-extension I engage the rounded extremity of the external splint in a sort of pocket which I make at the moment, by turning in for its entire length the bandage which I place round the pelvis; I also make a pocket for the reception of the internal splint which is enveloped by it in the usual way; the external splint is also adjusted in the ordinary manner, and the cushions being laid on, I close the apparatus; I lace and tighten the band; every thing is so arranged that the counter extension may be firmly made, when I make the extension on the extremities of the tape. The internal and external splints pass the sole of the foot by five or six inches more or less, but pass it by an equal length; each of them exhibit also the mortise and slope (*échancrure*) of Desault's splints, they being in fact the same. I then take the extremities of the tape directed, as we know, parallel to the limb: I bring each of them from their side, over the slope of the corresponding splint; I cause them to enter from without inwards by the mortise, and I tie them firmly together; they might be united by a buckle. In this way, the slope in the extremity of the splints performs the office of a pulley, the force it supports from the tapes tends to make the splints ascend, which are sustained, as I have said above, by the band and pocket: the counter extension takes place, and subsequently the extension and elongation of the limb. Such is the simple and really effective apparatus which I use to obtain continual extension of the lower extremity: its advantages are manifold.

The extension is made, not on the foot, as in Desault's apparatus, but on the entire leg by means of the rollers; it takes place in a direction parallel to the limb, and whatever be its degree, it is easily sustained by the patient. The counter extension is likewise made in a direction parallel to that of the limb; the laced band for the external splint, and for the internal one the *drap fanon*, sustained by three common ties of the thigh apparatus, present a resistance which was found sufficient in a robust and very muscular man, of whose case I shall give a brief history. Besides, as the extension thus made may be graduated at pleasure, I have the conviction now founded in fact, that it might be applicable in all cases. In fine, a circumstance which is greatly in favour of this apparatus, is that it consists of parts which the surgeon has always at hand, or which he may easily procure.

I had already used with success the apparatus for permanent extension above described, when I heard of a modification of Desault's apparatus made by M. Marcellin-Baumers, and described in the *Journal de Médecine de Sedillot*, tom. 24, page 29. We set out from the same principles, a thing easily conceived, they being no other than those of Desault and Bayer: but there exists between the method of M. Baumers and mine, differences sufficiently striking

for one to be preferred to the other: and I must say, mine appears to me much less complicated and more effectual. The matter may be readily decided. According to the *Journal de Medicine*, the parts of M. Baumer's apparatus are: 1°. a small band, intended to constitute a circular bandage on the foot and leg, down to below the ankles. 2°. Another band two ells long, broader and stronger than the preceding, which is to fix to the foot, the sole (*semelle*) of which more hereafter. 3°. A string of considerable strength, to support the upper extremity of the internal splint, about an ell and a half long, and turned back on itself so as to form a sort of noose, to which a buckle is attached. 4°. Two pads made of charpie, or of linen, to secure the parts from the pressure of the extending bands. 5°. Three buckles of such breadth and form, that the double strings may be received into them. 6°. A band from five to six inches in breadth, consisting of four doubles, the upper edge of which presents a sufficient resistance to the external splint, which is to take its point of support in a pocket expressly formed for it: a subfemoral (*unsous-cuisse*) made of several linen bands, its breadth equal to the width of two fingers, is attached to the band behind, and in a place corresponding to the tuberosity of the ischium: the band and the subfemoral must be fastened by means of buckles. 7°. Two splints, one external, the other internal: the external must be very solid, about three inches in breadth, and long enough to reach from the crest of the ilium to four inches below the sole of the foot: its upper extremity to be rounded towards the angles, so as not to tear the band (*le bandage du corps*) on which it is to be supported. At a finger's breadth from the lower extremity, there exists a quadrilateral mortise: in fine, towards the middle of its length nearly, there are to be two narrow mortises placed one by the other, and parallel to the length of the splint. The internal splint having the same breadth and the same thickness as the external, whilst setting out inferiorly on a level with the latter, is not to ascend farther than up to four fingers' breadth below the internal fold of the thigh. Sloped out (*echancrée*) at its upper extremity, it is to be arranged in every other respect as the external, with the exception of the middle mortises. 8°. Two other plain splints, thin and narrow, not going beyond the sole of the foot below; and extending up, the internal to about two breadths of a finger below the thigh, and the external as far as the upper part of the limb. These splints are employed only in cases of fracture of the body of the femur. 9°. A cross piece of wood, on which the internal and external splints are to rest by their lower ends. It may be made of wood sufficiently hard to resist the force of the extending power, and of such a length, that when in situ it may go a little beyond the splints; cylindrical in the centre, in order that the string may glide easily on them, it is to be quadrilateral for the remainder of its extent, and its extremities to be proportioned to the size of the lower mortise of the splints. 10°. A sole made of timber, thick and strong, in which are to be scooped three transverse mor-

tises, on which the extended string is to take its point of support. These strings (*ces lacs*) must be armed with buckles, the better to direct the extension, and to employ it as may seem fit.

The reader must have already remarked, that there are more pieces composing the apparatus of M. Baumer's than mine. As to its mode of application, it is easy to understand it, though the *Journal General de Medicine de Sedillot*, leaves us in ignorance as to the use of the mortises made in the centre of the external splint. Let us admit, as is probable, that they serve to receive the string placed in the upper part of the internal splint, and to carry over to the external splint all the force of the counter-extension. In my opinion, this is already a defect in the method; for let the internal splint which is so short, shift ever so little forward or backward, the string which connects it to the external splint will alter its length, and the counter extension which corresponds with the internal splint, will be lost: it is disadvantageous besides, to direct the entire counter-extension on the external splint, a circumstance which requires a much greater strength of resistance in the band (*bandage de corps*) which then supports all the force. The subfemoral splint (*le sous-cuisse*) furnished with pads, which lies near the ischium and margin of the anus, must cause considerable constraint by its size, and by the lateral pressure which it makes between the scrotum and thigh. M. Baumer's internal splint ascends only to about four fingers' breadth from the internal fold of the thigh, which deprives the limb of sufficient support at its upper and inner part, and obliges the surgeon to employ two accessory splints, of which one at least is very useless. Notwithstanding the precautions taken to render the extension supportable at the instep and tendo-Achillis, it is always the same system as that of Desault in nearly the oblique direction: and I am sure, that the direct traction must produce the same scars at the end of some days: on the contrary, in my apparatus, the traction is distributed over the entire of the leg, and unless the bandage be too tight, which causes a swelling of the flesh above its upper edge, there will not even be any excoriation. Again, the sole, the cross-wood in exact relation with the quadrilateral mortises of the splint, the subfemoral splint (*sous-cuisse*) the cushions at the instep, the numerous buckles attached to the apparatus, are matters not always at hand: and though I have no difficulty in conceding, that M. Baumer's method is more effectual than that of Desault's, and less complicated than M. Boyer's machine, I still maintain, that the complexity attached to it will prevent it from general adoption. In proof of which assertion I might adduce, the total oblivion in which it is buried in the *Journal General de Medicine*, since 1805. I still therefore think, that my method which combines simplicity and strength, and answers besides all the conditions of permanent extension of the lower limb: 1°. extensive and contra-extensive force parallel to the limb: 2°. the application of these forces over large surfaces on the leg and pelvis: 3°. a facility in graduating

the intensity of these forces at pleasure, will become of general adoption, and with this hope I publish it.

An athletic man had his right thigh fractured by a piece of timber, the fracture took place at the union of the middle and lower thirds of the thigh, it was very oblique from within outwards, and from above downwards. The displacement very great; I apply Sculley's bandage, supported by another around the pelvis; graduated compresses, small splints were placed at the projection of the fragments. A large bleeding subsequently repeated was had recourse to; entire rest and middle diet were observed. For a considerable time the upper fragment continued to project a little outwards, or rather the lower fragment was drawn in. At the end of seventy days, the apparatus was removed, and the fracture appeared solidified. The patient found himself quite well for two days, but the third day the callus yielded to the muscular traction, increased by the obliquity of the fracture: the thigh was shortened; the patient not being able to support the immediate application of the graduated compresses and small splints, I devise the plan of permanent extension above described. Every time the apparatus was examined, the limb was found to preserve its length and form. It was carefully kept on for two months. Applied on the 20th of December 1832, it was removed 20th of February 1833. Consolidation of the fracture perfect, and the limb exhibits no perceptible shortening.—*Ibid*.

*Calculus in a Child extracted by Weiss's Forceps; Extravasation of Urine; Cure.**—W. A. æt. 7, admitted October 17, 1831. He had had constant pain and difficulty in making water for more than six months. For two days there had been complete retention. A sound in the urethra detected a calculus one-fourth of an inch within the posterior boundary of the scrotum, where a small circumscribed painful swelling was felt externally. Several attempts had been made to hook it hence with a probe, and to favour its escape by dilating the anterior part of the canal with bougies, &c. The bladder was now much distended, the urine had been retained for forty-eight hours, there was much constitutional excitement, and the portion of the canal where the calculus lay was inflamed, tumid, and painful. Dr. M. had seen a similar case which nearly proved fatal, and Sir A. Cooper has seen two fatal cases of the same description.

“ Before attempting to grasp the stone with Weiss's catheter forceps, or to push it back behind the edge of the scrotum, and cut down upon it, I passed a steel sound along the urethra, to ascertain its exact situation. I then found that it was dislodged from its former position, and had passed back as far as the bulb,—probably by the pressure of the boy's fingers during the painful attempts at micturition. From the dilated state of the canal posteriorly, the sound was easily introduced into the bladder, pushing the stone before it. Although the urethra was now freed of its irritating cause, and

* Macfarlane's Reports.

there no longer existed an impediment to the discharge of the urine, yet I regretted that the stone was again lodged in the bladder, inasmuch as the operation of lithotomy, which might be required for its removal, was a more serious procedure than cutting down upon it in the urethra. It appeared, however, that the parts were in a favourable state for attempting to seize the stone, which was evidently of small size, and to extract it through the urethra. I therefore passed, with ease, into the bladder, a pair of catheter forceps intended for an adult; and on opening their blades, gave exit to a stream of urine, which from the small size of the catheter portion of the instrument, continued to flow for a considerable time before the distended bladder was perceptibly reduced. After moving about the expanded instrument in the bladder, I could not ascertain whether the stone was laid hold of or not, until I had partially withdrawn it. It passed freely out as far as the posterior edge of the scrotum, when its progress was arrested. I then discovered, by external examination, that the calculus was between the blades of the forceps, which were separated nearly one-fourth of an inch. As the narrowest part of the canal was still to be passed, I considered that the removal of the stone by an incision in the perinæum would be the safest practice. I found, however, on again attempting to withdraw the instrument, that the resistance was comparatively trifling; and as the calculus, so far as could be ascertained by external examination, was fairly embraced, and even covered by the blades of the forceps, I determined to continue slowly and cautiously to extract it. There was some difficulty experienced about the centre of the scrotum, and at the orifice of the urethra; but this was gradually overcome without force, and a stone, broken into fragments, was extracted. The patient complained but little of pain, and not more than three or four drops of blood were lost."

Dr. M. ordered an elastic catheter to be introduced as soon as the patient was placed in bed. In this the house surgeon failed, and at 12, P. M. Dr. M. found that only a small quantity of urine had been passed by the penis, and that there was swelling of the scrotum, which had commenced three or four hours previously. There was evidently urinary extravasation. *Large-sized catheter passed, and to be retained—scarifications—fomentations.* On the 18th the swelling and dusky redness had increased, and extended over the pubes. *More scarifications.* On the 20th the inflammation had extended over the pubes as far as the anterior spines of the ossa ilii. On the 21st the catheter was found obstructed, and the urine passed by its side. On the 23d he had an attack of convulsions, which did not recur, and next day a small stream of urine issued from an ulcerated opening on the dorsum penis, close to the pubes. On Nov. 12th he was dismissed cured.—*Medico-Chirurgical Review*, April, 1833.

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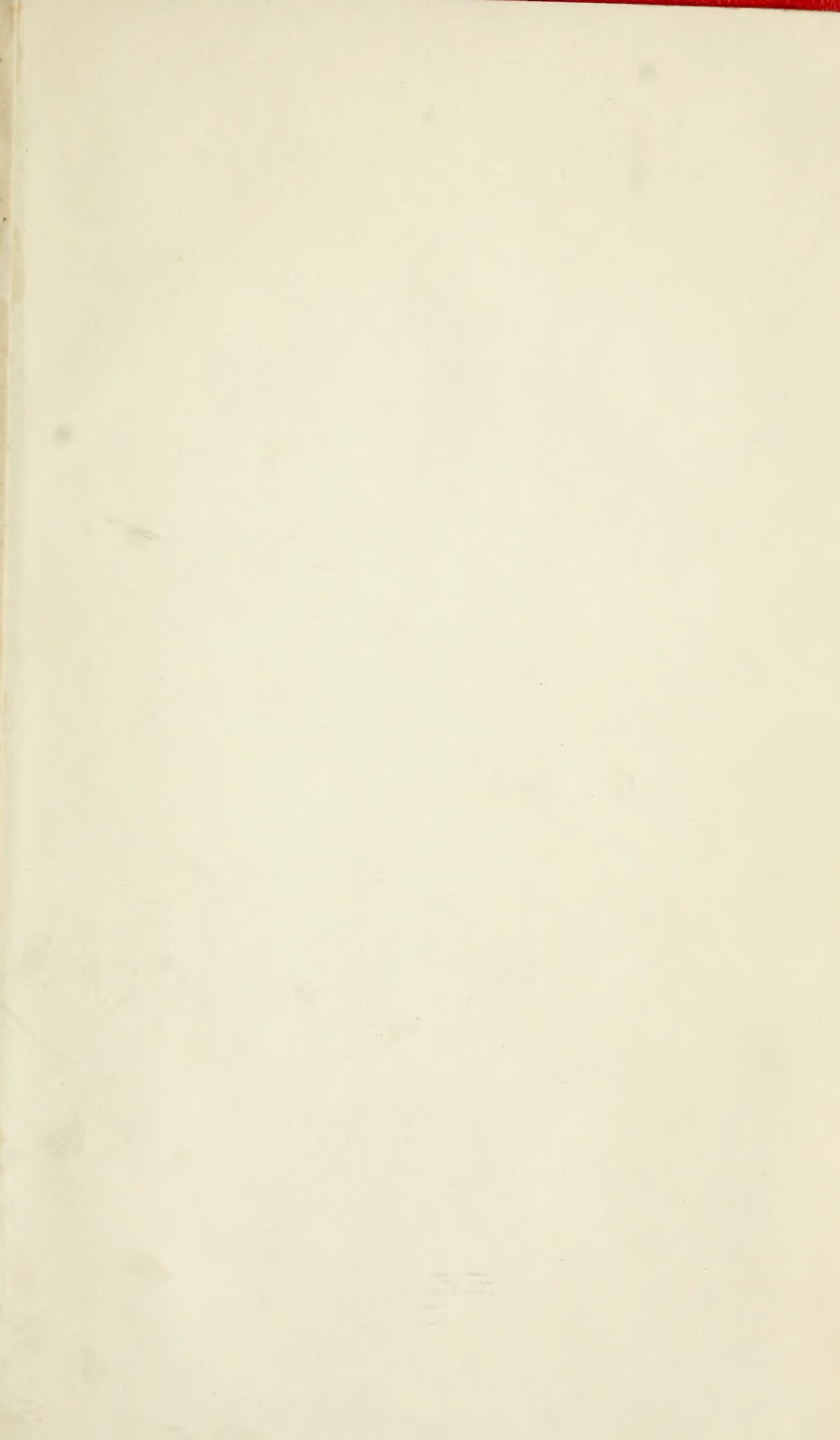
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